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LXXII NO. 6 JUNE 1951

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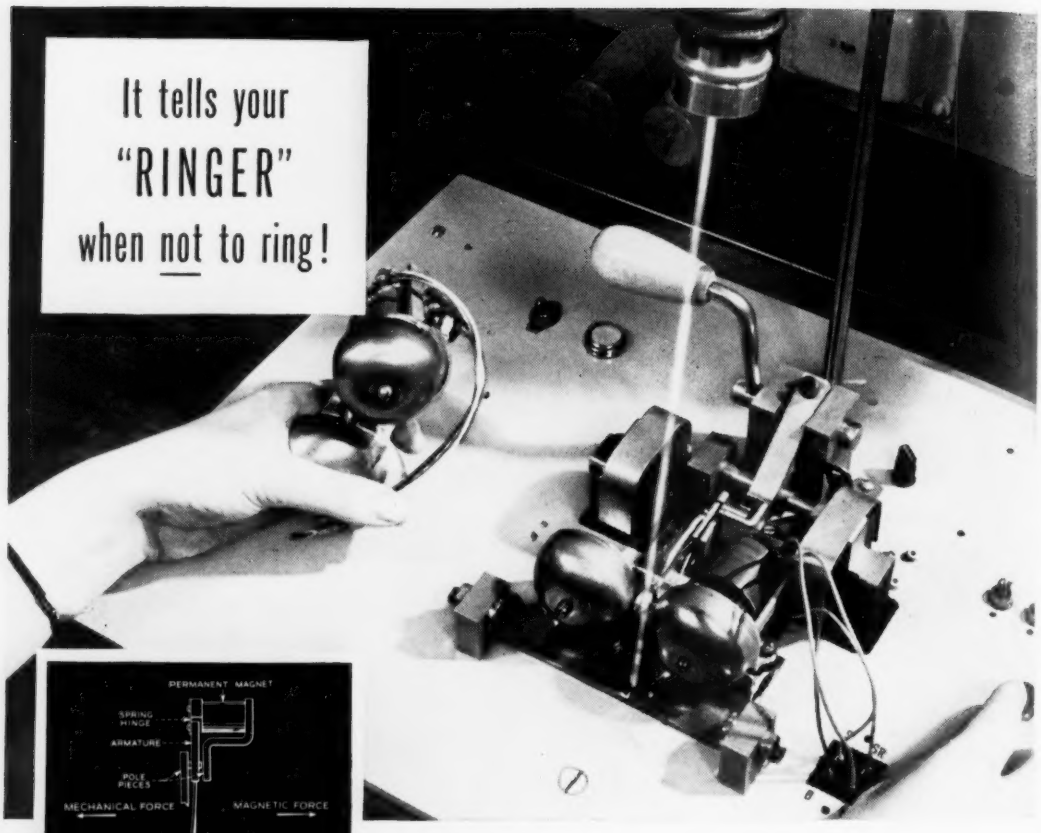

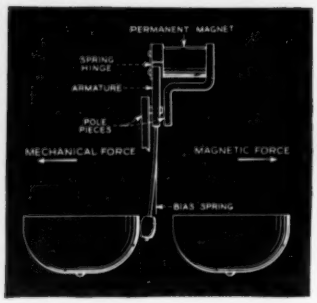

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THE SCIENTIFIC MONTHLY

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Science and Technology

(From the Month's News Releases)

Capper

A 4-ounce device about the length of a king-sized cigarette, which will uncap and recap a bottle with equal ease, is also a practical ice cracker. Since bottles may be refilled with other liquids and recapped, the gadget is useful for camping trips and other travel.

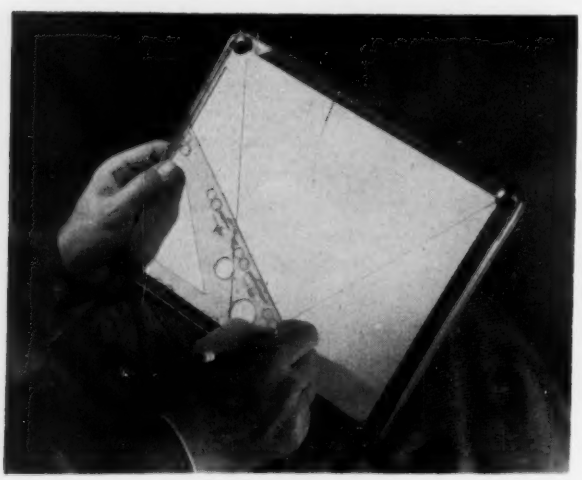
Collapsible Drum

A new 55-gallon reusable drum of synthetic fabric and rubber, suitable for shipping oils, greases, fats, acids, paints, emulsions, soaps, dry powders, and certain pharmaceutical and industrial chemicals, can be collapsed after emptying. More than 2,500 of the collapsed drums can be shipped in a standard box car that will hold only 300 rigid drums. The addition of a metal plate makes it possible to empty the drum from an overhead hanging position, rounded corners and light weight make it easily handled, and it needs no venting.

The Insects in Your Life

Garden insecticides should not be laid down in a mist on the outside of the plant. A new sprayer delivers a fine, fan-shaped, forceful spray under leaves, down, or sideways against walls, without soiling plants, and may be used for treetops. Fully loaded the sprayer weighs only 3 lbs. and delivers 4 gallons of solution when attached to a garden hose. A similar sprayer has a 2-gallon capacity.

Yosemite Chemical Company, 1040 Mariposa St., San Francisco 7, is marketing Dispercicide, a dry aerosol bomb for space-killing and residual control of adult moths, bedbugs, bean weevils, cockroaches, fleas, flies, gnats, grain weevils, midges, mosquitoes, silverfish, and spiders. It comes in a self-dispersing disposable container and acts in about four hours.



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See 9,000 Stars

A new model of the Zeiss planetarium projector, an instrument that has not been in production since before the war, has become available. Only a limited number of contracts can be accepted in any one year.

Title Shooter

A titler with complete accessory kit, for advanced amateur and T. V. film men, is available for 16-mm, 8-mm, and still cameras up to view camera size. It is designed for use in both a horizontal or a vertical position. Inexpensive, it is said to be equal in scope and application to much higher priced 35-mm professional titlers.

New Paints

A washable, streak- and mar-resistant flat wall paint comes in nine basic deep colors, which may be intermixed to achieve a wide range of decorator colors.

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Poison Ivy Remedy

A salve, based on zirconium carbonate, said to act as a neutralizer against the irritant in the poison ivy plant, has received Food and Drug Administration approval, and is reputed to be effective in about 90 per cent of cases. It also may prove useful in treatment of athlete's foot and some industrial dermatoses, as well as for a personal deodorant. Available in tubes and small jars.

Distinctive Fiber

Dupont's new polyester fiber, Dacron, formerly "Fiber V," will probably be used in suitings and all types of outerwear, particularly because of its wrinkle-resisting and press-retention properties when wet, moist, or dry. It launders with little or no shrinkage or wrinkling. Although a few problems remain to be solved—"pilling," for example, and dyeing—commercial production is scheduled for Dupont's new plant now going up at Kinston, N. C. Dacron is not chemically related to nylon.

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Publications Noted

Preparation for Retirement: A Study of Post-employment Adjustment. Esso Standard Oil Co., New York.

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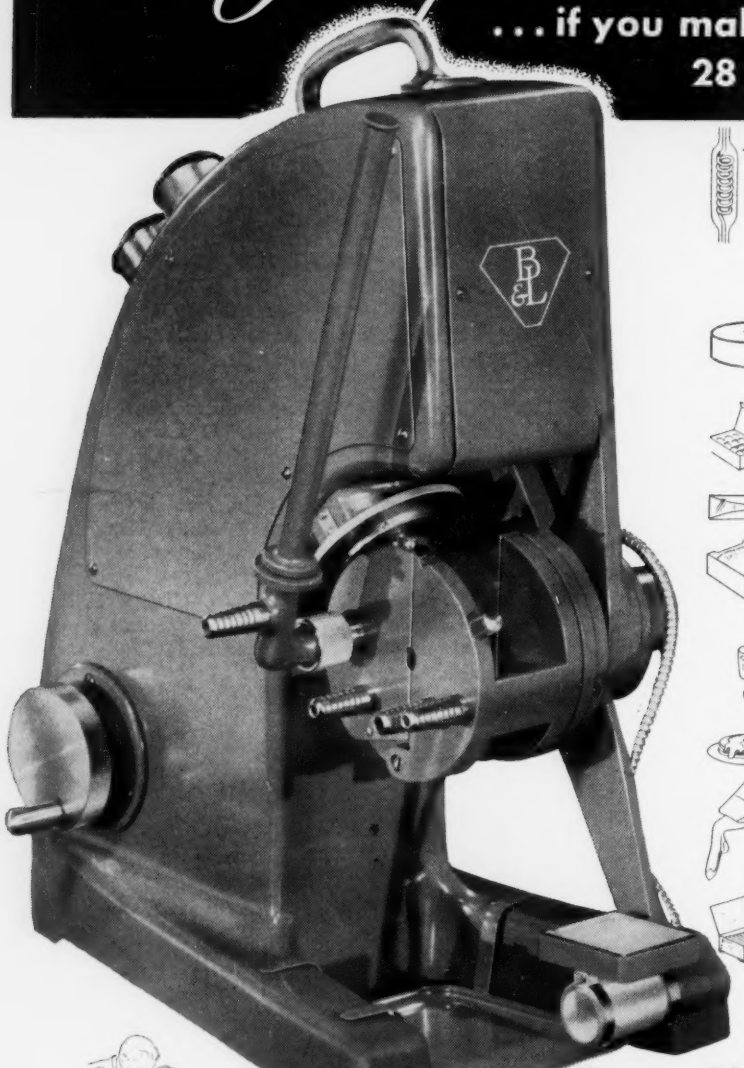

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THE
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JUNE 1951

Helminths in History

ILEEN E. STEWART

Ileen Stewart took her undergraduate studies in biology at the University of Manitoba, Winnipeg, and taught there in 1945. She holds an M.S. degree from the University of Minnesota in the field of parasitology. At present Mrs. Stewart is engaged in a breast cancer project at the Dight Institute for Human Genetics, University of Minnesota at Minneapolis.

SOMETIME about 1550 B. C. an Egyptian physician—or possibly several—gathered together the medical facts and fancies of the times and of the ages that had gone before in that region of the world. The collection developed into a book written on papyrus in the picture language we now call hieroglyphics and was eventually sealed into a dead man's tomb. Egyptologists digging for the buried past of the Nile Valley in 1872 unearthed the manuscript near Thebes. The history of parasitology begins with this find.

The year after it was brought to light the manuscript was purchased by an ardent archaeologist, Georg Ebers, who translated what he could, published this with a facsimile of the papyrus and deposited the original in the University Library at Leipzig. From Ebers' translation it was evident that the papyrus was a medical volume—essentially a collection of recipes for remedies against the diseases that plagued the early Egyptians. Digging and untombing continued in the twentieth century, and the vocabulary of hieroglyphic symbols increased. By 1937, when Ebbells in Denmark published his translation of the Ebers Papyrus, few words remained a mystery. Gradually a picture of the medical profession in ancient Egypt was emerging.

Specialization existed among the medical priesthood then as now. There are indications that, for a time, it was carried to such extremes that every disease was treated by a different physician. Generally, however, three categories of doctors practiced the healing arts. The surgeon-priest led the hierarchy. His duties were primarily concerned with wounds and fractures, common traumas of a young civilization. The unpleasant task of trepanning was his to perform. This brain surgery was not original to his age, for Stone Age skulls, 10,000 years old, show such scars. The Egyptian surgeon possibly improved the technique; with cautery he may have lowered the mortality rate—a little. Descriptions of the surgeon-priest's operations are preserved for us in the Edwin Smith Papyrus, dated approximately 1600 B. C.

A second branch of Egyptian medicine treated the patient's ills with incantations and spells. This practice was intimately connected with the religious life of the people and the divinities they had set up to preside over the curing of diseases. In the early dynasties Isis was the principal god; later Imhotep and Thot succeeded her in popularity. The priests in the temples of these gods left us a record too, and we read of their potions and recitations in the so-called London Medical Papyrus (1350 B. C.). The existence of this semireligious practice has given rise to the tendency, on the part of many historians, to consider most Egyptian medicine a matter of superstitious ritual. The incantations were limited in their application, however, and seemed to represent a combination of piety on the

part of the god-worshiping populace and a source of solace when the prognosis of a disease was discouraging.

What *Gray's Manual* is to the botanist today, the Ebers Papyrus must have been to the Egyptian general practitioner. The purpose of the compilation originally may have been to supply a text to the doctors who did not have access to the Royal Library. It is extremely unlikely that many physicians owned a copy, but the contents had probably been memorized during the training period at the medical school of the day. Dozens of remedies are offered for every common complaint. The doctor could make a choice or try them all in turn. Foul or offensive substances were part of most dosages, intended, no doubt, to make the prescription as unpalatable as possible to the evil spirit possessing the patient.

Two intestinal helminths of man are discussed in the Ebers Papyrus; the roundworm (*Ascaris*) and the tapeworm (*Taenia*). To "expel the roundworm in his belly" thirty-two recipes are available to the patient—recipes with such intriguing ingredients as pomegranate roots, potamogeton leaves, red ochre, malachite, turpentine, dates, and goose fat. In nearly all cases the final product was left overnight in the dew, strained the following morning and washed down with a liberal volume of "sweet beer." Possibly the chaser did as much for the welfare and recovery of the patient as the prescription that preceded it! "To expel the pains caused by tapeworms," a special recipe is given for a poultice to be applied to the abdomen.

A large section of the papyrus is devoted to the subject of haematuria, indicating the widespread incidence of this disease and its causative agent, *Schistosoma haematobium*. It was generally believed that the symptoms were due to the presence of "a god or dead man in the belly of a man," but another paragraph in the papyrus may be closer to the truth. "Another excellent remedy amongst those prepared for the belly is . . . and is eaten by a man in whose belly there are *ḥrrw*-worms; it is haematuria that produces them, and they are not killed by any remedy." Ebbells translates "*ḥrrw*-worms" as *S. haematobium*. It was not until 1851 that Bilharz discovered the adult schistosomes in the mesenteric vessels of a Cairo native and later demonstrated that the haematuria so prevalent in the population was caused by the eggs of these worms migrating through the blood vessels and bladder walls. That an Egyptian physician in 1550 B. C. knew of the existence of these minute worms in the blood vessels of his patients is extremely unlikely. He

Page from Eber's Papyrus discusses remedies for parasitic worms.

would never see the worms expelled in the feces, as were the roundworms and tapeworms. The religious custom of embalming the dead afforded the technicians in the House of Death an opportunity for human dissection; in this way they became acquainted with anatomy. There is a very remote possibility that the blood flukes were discovered during the evisceration process of embalming. That the physicians knew the "*ḥrrw*-worms" were not killed by any known remedy somewhat supports the diagnosis, for not until the antimony compounds were found to be specific for schistosomiasis (after 1900) was any effective treatment known. That *S. haematobium* was present in Egypt at the time of our author has been proved recently in another manner, however. Ruffer in 1910 was able to identify calcified eggs from the kidneys of two mummies from the thirteenth century B. C.

Another helminth parasite that seems to have harassed the ancient Nile dwellers was the Old World hookworm (*Ancylostoma duodenale*). The following description is believed to be that of Egyptian chlorosis, the disease caused by this parasite. "If thou examinest a man for suffering in his cardia and thou findest that he had turned deathly pale (lit., 'has crossed the channel to the beyond'); he has pains in both his sides, his belly is too narrow for food and his stomach is oppressed when (some-

thing) enters into it (this thou shalt say) it is *ḥtw*-worm; thou shalt fight it by means of strong remedies." The reference to the patient's pallor could well allude to hookworm anemia, a common and severe symptom of this disease.

Toward the end of the papyrus there is a short appendix of surgical operations. One of these is entitled "Instructions concerning *ʿ3wt* in any limb of a man" and states: "If thou findest a swelling of a larvae in any limb of a man, then thou shall apply a bandage to it. If thou findest that it comes and goes, piercing through to the flesh which is under it, then thou shalt say concerning it, *ʿ3wt* has entered it. Thou shalt perform an operation for it, the same being split with a knife and seized with an instrument [forceps]; . . there is one therein, . . like [brains] of a mouse; thou shalt remove it with a knife without taking away those enclosures [the fibrous capsule] which are on the sides of it and which adjoin the flesh."

The translation of *ʿ3wt* is taken no further than "larvae," but it seems highly probable that the original author was referring to the Guinea worm (*Dracunculus medinensis*), which is still endemic in the Nile Valley. The operation described above for its removal is the preferred treatment today. However, the generations that followed these early dynasties lost much of the art of surgery that had reached such proficiency at this time. Later there arose a new method for removing this irritating pest. The worm was wound on a weighted stick which was turned a few centimeters every day; eventually, if the parasite did not break, it was completely removed from its subcutaneous burrow. Just when this treatment became established is unknown, but it is still practiced by infected natives in Africa and Asia.

While the Egyptians were constructing their pyramids and leaving the Sphinx to watch the desert, another race in that region of the world was becoming acquainted with parasitic helminths. This group left their report of the struggle in the Old Testament and, later, in the Talmud. The sojourn of the Hebrew people in Egypt had been a lengthy and laborious one, and their first contact with many parasites occurred during this period of their history. Their choice of a leader for the escape from Egypt and the long journey through the desert was an expedient one. Moses (1531–1450 B. C.) had spent his youth at the Egyptian court as the foster-son of Pharoah's daughter. Here he probably received the best education available and learned much from the court physicians; as leader of the Israelites this medical knowledge proved invaluable.

The poor health of the traveling tribes may have warranted the pause that was made in the journey when Moses delivered the Laws to his people. In Num. 21 : 6–9 he speaks of the "fiery serpents" that were to be "put upon poles." The exact interpretation of this passage is still in dispute, but most historians are willing to agree that the Guinea worm was the serpent in the camp. Very likely the pools where the Israelites had stopped to bathe or drink contained infected *Cyclops* and an epidemic had broken out in camp. Moses then instructed his followers in the primitive operation of winding the worm on a stick. It is estimated that the Children of Israel took about twelve months to make their famous journey; this time approximates the ten-to-twelve-month development period of the Guinea worm.

All animals to be used for food by the migrating Hebrews were separated into "clean" and "unclean" categories, based mainly on infection of the animals with parasites. Swine, on the condemned list then as now, were sources of pork tapeworm and *Trichinella.* Most of the "unclean" fowl were scavenger birds of prey. Fish were acceptable in the diet, but not other seafoods. Had a parasitologist been preparing this list, this last law would have been reversed. One major helminth was allowed free access to the Hebrew table; the beef tapeworm, *Taenia saginata,* continued to make its presence known.

At another time Moses warned his people against "infected water," instructing them in drinking from cupped hands. Reasons for this warning are not given, but some can be offered. Many pools in the tropics are filled with small blood-sucking leeches that tenaciously attach themselves to the pharynx of man and beast. Just as many watering holes may have been sources of the Guinea-worm larvae, so the same waters may have swarmed with the cercariae of the schistosomes.

None of the Old Testament sources is complete or concise about the parasitic travails of the Hebrews. But they do furnish us with information about the probable helminths that the Israelites acquired in Egypt and carried to the Promised Land in Asia.

In later periods of Hebrew history the Greek influence in medicine overcame the Egyptian. The Talmud is the compilation at a later period (A. D. 390) of the new interpretations and explanations of the old Laws. From the medical contents of this work we find two references to helminths. The first is the consolation offered to the patient that hydatids of the liver are not fatal; the second, that onions should be eaten to rid oneself of worms.

Eastward, in India, at the time of Moses, a quite advanced civilization existed, one with a broad knowledge of medicine. There appears to have been no specialization here—all physicians were general practitioners. Most of the early medical writings were incorporated into the sacred books of the various religions. In the Vedas (1500 B. C.) the physician is informed that: "Abdominal colic, expectorations, rigors, pain, eructations, vertigo, disgust for food, redness of the eyes and emaciation may arise when the disease depends upon worms." Indian medicine has virtually remained stationary at this degree of development since the Vedas and similar volumes were written.

Much of the medical lore of the Egyptians became the heritage of the Greeks as they fashioned their civilization in the last few centuries B. C. at the eastern fringe of the Mediterranean. The knowledge they inherited was essentially factual, the accumulation of Egyptian observation and experience. The Greeks attempted to put these facts together and derive a systematic pattern in nature. Many of their interpretations were still tinged with mysticism, but were philosophical and logical as the Egyptians' had never been.

For many generations the Greek nation had no organized priesthood, as the Egyptians had, to protect and propagate their learning. Nor had they an easy means of written communication. In papyrus the Egyptians had found a cheap writing material, as had the Babylonians in clay tablets. But the rocky shores of Greece and Crete offered only metals and parchment for stationery. Both were expensive and inconvenient.

Until trade was established with the Egyptians through the Phoenicians and papyrus was imported, the Ionian scholar was obliged to use what his natural resources could yield. The lack—or scarcity—of writing materials and the general illiteracy of the population led the adaptive Greek philosopher to devise the poetic expression; short poems could be easily memorized, even sung. The dissemination of his ideas set the scholar on the road. If he became popular or renowned for his wisdom, he rested and established a school.

Medicine, in the early Homeric era of Greek history, was ecclesiastical, in the temples of Aesclepius. Here a combination of empirical knowledge and superstitious ritual was foisted upon the ill Greeks who sought a cure from the gods. Not until the fifth century B. C. was medicine removed from this environment and placed on a secular plane.

During this fifth century, from the medical center of Cos, Hippocrates (460–377 B. C.) emerged to become, historically, the father of medical science. Greek medicine was slowly liberating itself from religion when he began to practice, but more than any other physician of that day, he assisted in the complete severance. He attempted to keep medicine free from philosophy as well as religion, and to place upon the medical profession a cloak of ethics it had never hitherto worn.

Hippocrates' fame is dependent primarily on his literary achievements. By general consent, however, only four treatises in the immense Hippocratic collection can be credited personally to this pioneer scientist. The remainder of the multitudinous papers are the productions of a score of fifth- to second-century authors.

In the surgical sections of the Hippocratic papers can be found the description of an operation for the removal of hydatid cysts from man, confirmation that the parasite *Echinococcus granulosus* preyed upon the early Greeks. It is doubtful if Hippocrates knew that this disease was in any way related to a helminth, but he did know that flatworms and roundworms were harbored by his fellow-men. In his *Aphorism* he speaks of these parasites as the only intestinal forms in man. Also in this volume we read: "And indeed roundworms do breed, but the broadworms do not, though some say they do." With this statement we meet the first speculation about the origin of parasitic helminths. For the next 2,000 years this problem vexed the minds of all naturalists who looked upon the wiggling worm and asked "From where?"

To account for the broadworm that did not breed, Hippocrates offered a novel hypothesis: "'Tis bred in the Child in the womb, and grows old with the Man (not occasioning his death) unless 'tis first wholly expelled by medicines." His curiosity about tapeworms was not limited to speculation, for he observed: "For a man with a broadworm exerns often with his Dung, something like a cucumber-seed; and there are some men who say this is the offspring or brood of the worm. But they who say so, do not say right in my opinion; neither can so many be bred by one creature, nor is there room in the gut for so many of them to bring up the brood."

From the habit the beef tapeworm has of shedding proglottids singly, it would appear that ripe fragments of this species were the "cucumber-seeds" to which Hippocrates referred. The anthelminthics recommended were, in most cases, identical with those offered by the Egyptians in their papyrus: further proof that the Greeks claimed and built upon the older knowledge from the Nile region.

Aristotle (383–322 B. C.) followed Hippocrates in drawing attention to human helminths. In his *Historia animalium* he verifies observation made by Hippocrates—"The flatworm . . . lays a thing like a melon seed"—and adds a new species to the existing total of two found in man.

Other insects are not derived from living parentage, but are generated spontaneously; . . some in the flesh of animals, some in excrements; and some from excrements after it has been voided, and some from excrements yet within the living animal, like the helminths or intestinal worms. And of these intestinal worms there are three species. One named the flatworm, another the roundworm, and a third, the ascarid. These intestinal worms do not in any case propagate their kind.

Not even a preformation theory such as Hippocrates proposed was acceptable to this author. But here we do meet the theory of spontaneous generation, which was to account for reproduction in lower animals for centuries to come. Aristotle allowed sex and parenthood to man and the beasts. In fact, he even extended the privilege to the grasshopper and the squid. But, for the father of the scientific method, worms could only come from putrefying material. It is interesting to read Aristotle's apology for being so fascinated by these forms. "In all natural objects there lies some marvel, and if anyone despises the contemplation of lower animals, he must despise himself."

Pliny (A. D. 27–79), a later Greek writer, was the first to mention the size of the flatworm, to which he gave the name *Taenia.* His surprise that such a creature would reach a length of thirty feet in the bowels of a man is promptly followed by several nauseating vermifuges for the elimination of the monster. Possibly the foundations of scientific accuracy were laid when Pliny took the time to straighten out and measure this early tapeworm.

The last great biologist of antiquity was Galen (A. D. 130–200). His immortality was gained by splendid anatomical manuscripts, which became the textbooks of medical men for generations, until an even greater anatomist, Vesalius, corrected the errors in Galen's dissections. Galen's contribution to parasitology was to locate the site of the helminths in the intestine. He believed that the ascarides (pinworms) bred in the lower part of the gut and migrated to the anus on occasion. The roundworms lived in the upper part of the intestine and often could be accused of wandering into the stomach; the recovery of *Ascaris* from the vomitus of children may have prompted this statement. The broadworms were understood to extend throughout all the intestine.

At the same time that Galen wrote of worms, a lesser Greek author, Atius, very ably described the symptoms of intestinal helminthiasis and contributed another theory of origin.

The worm that is called the broadworm . . . is the changing of a small skin within the intestines into a certain living body, that causes continual gnawings at the stomach, and an insatiate appetite to eat. For that which is received is not digested but purg'd away. Leaness and weakness of the Body and a continual Hunger attended with Laziness, are the Infirmities which follow. For that which lives in the Intestines, consumes the Aliment as soon as receiv'd and afterwards gnaws, or presses upon the solid substance of the stomach and guts; But the truest sign is when something like Gourd Seed is voided with the excrement.

For several centuries after the Greek civilization collapsed under Roman invasion, little progress was made in the natural sciences. The peak that biological science had reached was not achieved again or surpassed until the nineteenth century. There were numerous Latin authors during the period of the Empire. Some wrote of parasites. But those who did added little that was new. They discovered no new forms in man and merely reiterated the older beliefs that the worms originated from membranes and "humours."

In the Far East the Chinese people left an interesting literature concerning their aquaintance with parasites. The earliest translation is dated about 90 B. C. Most frequently, nine worms are listed as intestinal forms in man, but some of these are fantastic varieties, more figments of an imaginative mind than likely living varieties. "Of the nine worms, only *Huei ch'ung* ('*Ascaris*'), *Wei ch'ung* ('stomach worm') and *Ts'un pai ch'ung* ('tapeworm') do not change their form while the remaining six worms frequently transform under the influence of evil effluvia and cause phthisis which when generalized in the form of *Ch'ung chu* ('worm-spreading disease') may infect the nearest relatives of the patient." The "six remaining worms" are illustrated in the shape of turtles, crabs, mantids, and frogs.

Any Chinese manuscript dealing with intestinal worms devoted considerable space to theories of origin. One of the commonest theories was described by Chang Chi in A. D. 217.

Foodstuffs which are not easily digested such as raw minced fish, preserved meat, insufficiently cooked pastry, hard-boiled rice, etc. are liable to become stagnant in the stomach. When influenced by moisture and heat, they become sticky and coagulate. When coated with the warm blood and nourished by the vital elements of the host, they all metamorphose into living creatures with heads and tails in the shape of worms. From foodstuffs they have originated and on foodstuffs they are now to feed for their nutrition.

Because of the relatively recent discovery that raw fish and uncooked meats were sources of hel-

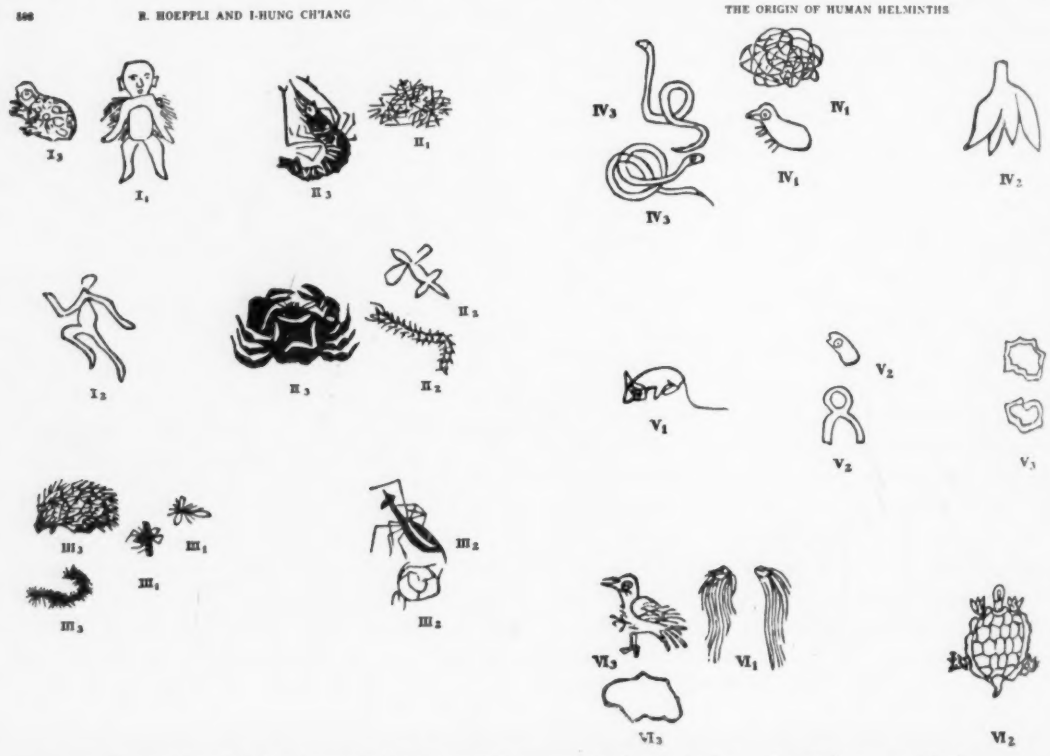
minth infection, these early quotations read like predictions. The reference was probably pure chance, but it was repeated by several authors. It may have been that the correlation between the development of the helminthiasis and the patients' gastronomic habits was obvious even to these early physicians.

The theory that parasites were inborn, which was prevalent in Europe (originally in Hippocrates' writings), was rarely found in Chinese literature. As early as the twelfth century, however, the following brilliant guess was made: "Some people become parasitized by worms through eating fruits and vegetables or animals' viscera, which contain their progeny." The truth of this statement had to wait another four centuries for positive proof. Several decidedly different vermifuges were recommended by the Chinese physician: asafetida, betel nuts, quicksilver, chloride of mercury, and vermilion. Treating intestinal helminths would appear to have been an effective method of very speedily joining one's ancestors.

In the Near East, during the years of scientific decadence in Europe, several prolific writers of medical fact and philosophy appeared. The earliest of these was Rhazes (A. D. 850–932), who wrote nearly 250 treatises. His fame rested on being able to distinguish between smallpox and measles, and he was the first to recommend animal gut for suturing abdominal wounds. His only contribution to parasitology was the description of a female patient suffering with more than forty Guinea worms. In recognizing the cause of the sinuous swellings on her body he was ahead of the pathological teachings of his day. Generally the condition was held to be due to an irritated nerve.

The Persian physician who most influenced Western thought was Avicenna (A. D. 980–1037). His *Canon of Medicine* took its place with Galen's manuscripts in medical literature. Because of the Koran's stict veto on human dissection, Avicenna was able to contribute little more than had Galen in human anatomy, but his speculations in physiology surpassed those of all his predecessors.

Four kinds of helminths were described by this Arabian scholar: the long worms in the small intestines; the "pumpkin seeds" which he knew were acquired by eating raw beef; small worms which migrated from the anus and caused intense itching; and a fourth kind, the roundworms, which were malignant in the damage they caused and the severe symptoms they produced. Although Avicenna recommended no anthelminthics that were not known to the Greeks, he was the first to correlate the worms to the symptoms produced and to distinguish between a drug that killed the worms



Some illustrations of the hypothetical parasitic worms believed in by early Chinese medical men.

and a purgative. Despite the fact that Rhazes had demonstrated the true character of Guinea-worm infection, Avicenna continued to uphold the idea that the worm was only a ruptured nerve.

The Renaissance which followed this lengthy period of inactivity in biology did not arouse European naturalists to spend much time on parasites. Possibly they felt at home with their "guests" and were as resigned to their presence as to hunger and the plague, the Inquisition, and no plumbing. Intestinal worms were still considered to be natural products of the body; they were merely more curious than the other products. A few physicians, scattered in a dozen countries, maintained sufficient interest to record the unusual about these animals. By the fourteenth century the cucumber seeds of the Greeks had been identified as sloughed-off parts of the intact worm. By the sixteenth century the silent tapeworm had given rise to a new argument. The segmented nature of the creature had become apparent. The possession of individual pores and an internal structure had been noted. Could the thirty feet of animal be one worm? Or was it many small ones strung together like beads? Because some authors could see the longitudinal connecting strands of the excretory and nervous systems, they took the side of unity. The hunt was on for a head and tail. Most authors never recovered a complete worm. In fact, most authors did not know which end was which. For some time the narrow neck end of the worm was considered hindmost, and on the surface of the last proglottid several naturalists described and drew eyes, two nostrils, and a mouth. Eventually Michael Fehr (1644) recovered a complete Taenia and into the literature came the first correct description and drawing of a tapeworm scolex. The argument finally rested.

By 1600 Spigelius and Tyson had distinguished between the gourdworm and the so-called Taenia. From illustrations these two scientists made it is apparent that the gourdworm was a *Taenia,* very likely *T. saginata,* and the "Taenia" really *Bothriocephalus.* Excellent drawings also existed, thanks to several authors, of *Ascaris* and *Enterobius*; details of their anatomy had to await the invention of the microscope.

Several authors prior to and after 1600, had written of parasites from game birds and animals and indicated their similarity to forms in man. The outstanding contributor in this field was Francesco Redi (1626–97), a colorful personality who is given the credit for fathering the science of parasitology.

Early in his career, Redi, with his friend Cestoni, made a very practical contribution to the welfare of the human race. He discovered the cause of itch. That the guilty mite existed as male and female was demonstrated, the methods of transmission were clarified, and the curative (?) powers of mercuric compounds made known. As with so many important discoveries, it lay in a library for the next one hundred and fifty years while *Homo sapiens* scratched his way about the world.

Redi's admission to the biological Hall of Fame, however, is for another experiment, the results of which had more far-reaching effects on the thinking of the scientists who followed him. His discontent with the widely accepted theory of spontaneous generation led him to conduct experiments with insects which established parenthood and

Medal of Francesco Redi and his signature.

clearly refuted the theory for these animals. It may have been his success with these experiments that induced him to consider parasitic worms, also generally considered to derive existence without parental means. Any of his doubts about their origin must have been dispelled when he discovered reproductive organs and eggs in *Ascaris*.

The enthusiasm that marked his earlier efforts continued in later life, but he became a collector instead of an experimentalist. His industry in obtaining parasites was tremendous—not even a dead animal in the zoo escaped him. He discovered and described a total of 108 species—specimens from every phylum and major group into which parasites are divided today. It is for this effort that he is called the father of parasitology.

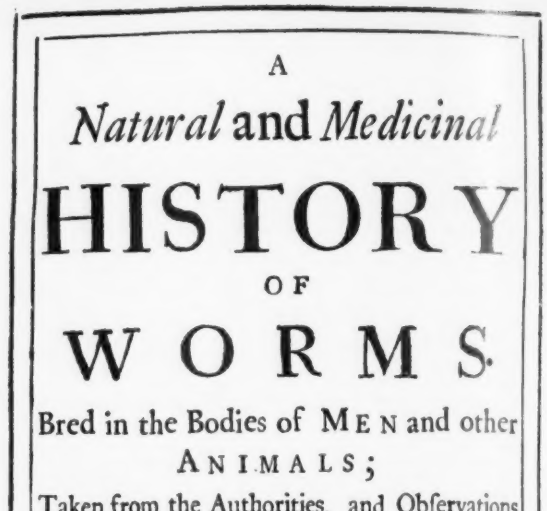
Despite Redi's contribution of sexing parasitic worms, the controversy over their origin continued until the latter part of the eighteenth century. Fact overcame fiction very slowly. As late as 1788 leaders in the field were still upholding the decaying theory of spontaneous generation.

The discovery and development of the microscope early in the seventeenth century provided fresh incentive to parasitologists in the investigation of their wormy subjects. One of the earliest microscopists, Marcello Malpighi (1628–94), turned his lens on intestinal helminths and left us a drawing of a *Taenia* head which exhibits human features. Among Leeuwenhoek's (1632–1723) varied manuscripts can be found some describing his introduction to helminthology.

> A certain physician lately shew'd me some small Parts, . . . which he thought were worms. . . . I inspected them with a Microscope and judged them to be Parts of worms . . . and when I separated those parts which were broad, there came out of the broken Parts, a great and almost incredible Quantity of small Clots. These Clots were a little larger than those which render our Blood red, and they were as equal in their Proportion as Shot.

This inspection of what were obviously eggs of a tapeworm whetted his curiosity. Several interesting letters exist telling of his discovery and examination of worms from fish.

Progress in parasitology with the aid of the microscope was rapid. But the discovery of life cycles and the diversity of the intermediate hosts, of immunity and epidemiology, is a modern legend and not part of this early history. As this story started with a book, so it ends with one. In 1721 a little volume appeared, entitled *A History of Worms*. It was written by a Dr. Le Clerc, translated from the Latin into English, and published in London. It may well be that this manuscript is the first—and only—such history of worms to be published. It was written at the end of a particular


A

Natural and *Medicinal*

HISTORY

OF

WORMS.

Bred in the Bodies of MEN and other ANIMALS;

Taken from the Authorities, and Obſervations of all AUTHORS who have Treated thereof, from *HIPPOCRATES* to this Time:

TOGETHER

With an Enquiry into the Original of WORMS, and the REMEDIES which deſtroy them, with a particular *Formula* of *Medicines* adapted to the USE of FAMILIES, and Illuſtrated with ſeveral Copper CUTS.

Done from the Latin of D. Le CLERC, M. D. By the *Compiler* of the General HISTORY of DRUGS, from *Pomet*, *Lemery*, *Tournefort*, &c.

LONDON:

Printed for J. WILCOX, at the *Green-Dragon*, in LITTLE-BRITAIN. M.DCC.XXI.

era in scientific thought and before our modern age began. In the dedication to His Grace, The Duke of Montague, Le Clerc summarized his treatise:

> The subject is medical and natural History, which of Right claims your Grace's Patronage, and Protection. In the following sheets, my Lord, you will meet a Library of Learning abstracted from the Ancient as well as Modern Greek, Latin and Arabian writers; Besides which the Compiler of History hath trac'd the Spanish, French, English and Italian Authors, among whom your Grace will take pleasure in the laudable Discoveries of the accurate Vallisnerius and Redi, the indefatigable pains of our own Tyson, the minute microscopical observations of Lewenhoeck, Harsoker, Morgagni etc, the industrious inquiries of Schel-hammer, Schenkius, Malphigius, all who together, with many more Eminent learn'd Physicians and Naturalists have contributed to this useful as well as curious Work; wherein the most exquisite and Ingenious Vallisnerius makes the Origine of Worms, Coequal with that of Adam, and says, that the Eggs of all those Worms or Insects that breed in the Body of Man, were at first created in the Body of Adam; nor were they excluded before his Transgression.

The Age Problem in Research Workers

Physiological Viewpoint

N. W. SHOCK

This article and the three that follow are based on a symposium held by the Industrial Research Institute in October 1950. Dr. Shock (Ph.D., Chicago, 1930), chief of the Gerontology Section, National Heart Institute, of the National Institutes of Health, since 1941, is also physiologist for the Baltimore City Hospitals. His A Classified Bibliography of Gerontology and Geriatrics *and* Trends in Gerontology *have recently come from Stanford University Press.*

AGING is a problem common to all of us. It is a process that begins with conception and continues throughout life. In the past most people have regarded aging as something like the weather—something to be talked about but about which nothing much could be done. There have also been a great many recommendations about appropriate formulas for increasing longevity. As early as 1780, Luigi Cornaro[1] published a book on how to live to be 100. Since that time many others have added prescriptions—or restrictions—that they believe will lengthen life. Some have recommended eating nothing but raw meat or raw vegetables. Others have suggested goat milk, yogurt, etc. At the present time there are those who believe that after the age of 40 one should avoid milk and eggs in the diet. Similarly, the ill-effects of many pleasures, including smoking and drinking, have been held up as factors limiting longevity. However, none of these recommendations has been subjected to rigorous scientific tests.

The problem for discussion—"What is physiological aging, and how does it influence research productivity?"—poses a double-barreled difficulty. In the first place, we need to consider the question of how physiological characteristics influence mental performance in general and, second, how these may change with increasing age. Although we are convinced that the brain is an essential part of the body and that creative activities are dependent upon a well-functioning brain, the precise manner in which physiological conditions within the brain influence behavior and productivity has not been worked out in any detail. We may, however, depend on the validity of the common-sense assumption that intellectual productivity is in some way dependent upon bodily vigor. How, then, does bodily vigor change with age, and what are the conditions that maintain it?

What, then, is aging? Since all animals, including unicellular ones, age and die, we make the general assumption that the aging process is fundamentally one of cellular biology. However, very little is known about the characteristics of aging cells.[2] In fact, most of the studies of cellular activities in old animals fail to show anything strikingly different from similar functions observed in young ones. With the development of new techniques and greater knowledge of enzymes and the chemical processes going on within cells, we may look to substantial advances in this area in the future. On the other hand, when we examine aggregations of cells that are put together into tissues and organs for specific functional purposes, we do find definite differences between old and young tissues. The most striking difference is that in the old tissues there is an accumulation of extracellular materials filling up the spaces where functioning cells have disappeared.[3] Studies of the functioning of separate organ systems have shown that the organs of older animals in general tend to perform less well than those of young animals. It should be pointed out that the primary difference observed is often a reduction in reserve capacity in the old animals; that is, they are able to meet the day-to-day needs in an adequate fashion but, when called upon to perform under conditions of stress or extra load, they are unable to meet the demands as well as young animals.

This is not the place to indulge in a detailed review of research on the physiological aspects of aging. Review of this work, as well as our own studies, leads us to three general conclusions that I

believe are of importance: Aging is a gradual, continuous process, the rates of aging of different organ systems may vary widely within the same individual, and there are marked individual differences in the rate of aging in humans.

Aging is a gradual, continuous process insofar as we now know it. Unfortunately this conclusion must be based on measurements made on different people at different ages. These observations all show a gradual loss of efficiency or performance, oftentimes beginning at relatively early ages and continuing to the highest age levels.[4] For example, the maximum elasticity of blood vessels is reached at the age of 14 or 15 years and diminishes gradually thereafter.[5] Although the rates of change with age differ markedly for different functions, the average changes are all gradual. In some functions there is a long plateau throughout middle life when little change appears, but in no instance is there a precipitous drop beyond a certain age. These observations do not exclude the possibility that within an individual age changes may occur rapidly. This question can be answered only when we have observations made serially on the same individual as he grows older. Opportunities for this kind of research require the cooperation of active middle-aged people who will serve as experimental subjects over their life span.

The different organ systems in the same person do not all age at the same rate. In our studies in Baltimore, evaluations of the functional capacity of different organ systems have been carried out in older people.[4] We have not been able to demonstrate any close relationship between rates of aging in the kidney, the heart, and blood vessels, metabolism, nerves and muscles, perceptual capacities, and mental performance. Our most extreme deviate is a man who claims to be 91 years old. He has a kidney function equal to the average 60-year-old man, a nerve conduction time of the average 30-year-old man, perceptual capacity of the average 80-year-old man, but the metabolism of the average 90-year-old group. This person represents an extreme complexity of the aging process, but most individuals show some discrepancies.

There are wide individual differences in the rate of aging in different people. All our studies have shown that with increasing age individuals become less similar. For instance, the range in values for kidney blood flow in 30-year-old adults is roughly 900–1,200 ml per minute, whereas in the 80-to-89-year-old group, the range is approximately 200–800 ml per minute.[4] We have also observed some men in their eighties whose effective renal blood flow is as great as the average of the 40-year-old group. Similar individual differences have been shown in tests of mental performance as well as of physiological factors.

What are the implications from these findings? First of all, it is clear that there can be no answer to the general question "When is a man old?" If this question is to have any significance, we must always say, "Old—with respect to what performance?" Since many performances are highly specific, it may well be necessary to resort to an overall evaluation, subjective though it may be, to answer this question for a particular individual. This conclusion also has important implications for determining appropriate ages for retirement. It is obvious that dependence upon strict chronological age can have no real meaning.[6] All that can recommend chronological age is the adminstrative convenience in its application. However, with increasing numbers of elderly people, and the need for maximum productivity to maintain our economy, it is highly questionable whether we can afford the waste involved in discarding effective employees simply because they have obtained a given chronological age.[7] Our most important problem is to devise ways and means of maintaining the effectiveness of older people so they can be retained in their positions. Research productivity in itself has never been closely correlated with physiological states. We can, however, regard the maintenance of physiological vigor and health as an aid to full productivity and mental alertness. This is primarily a problem of personal hygiene, with perhaps the added advantage of periodic health examinations to detect early stages of disease and pathology that are more easily remedied then than later.[8] The most productive research workers are notorious in disregarding fundamentals of hygiene. Long hours and work under pressure may or may not result in the development of gastric ulcer, coronary artery disease, etc. Nevertheless, certain broad principles of hygiene are undoubtedly of value in maintaining health and vigor. Research workers should be given opportunity and encouraged to follow these principles as we now know them.

A problem of identifying and describing age changes in human beings is one that should receive the attention of industrial research in the future. From its early beginnings, where industrial research was concerned primarily with specific practical problems, there has been a gradual shift to emphasis on fundamental research. Experience has shown that the building up of a backlog of fundamental research is of great value to industry in developing new and better products. There is

need now for industrial research to turn its attention to the producer as well as to the product. Many important problems relating to the productivity of older workers can be solved only within the framework of industry. Important questions about the changes of attitudes toward work with advancing age, as well as the effect of programs designed to train people for retirement, should also be considered. Nor should the physiological aspects of aging be neglected. With increasing stability of workers within plants and industries, serial examinations on the same individual as he ages should be made by industry itself or in collaboration with university scientists. The questions of middle age and later maturity can be answered only with the active assistance of industry where large groups of people are actively working and producing, just as our knowledge about growth and development of children resulted from cooperative studies with schools and universities. As our population ages there will be greater need for this fundamental information. It offers a real challenge to research.

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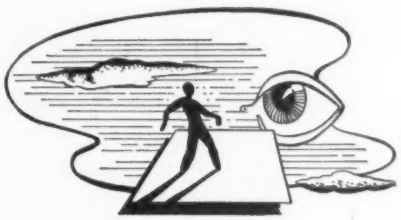

Psychological Viewpoint

JOSEF BROZEK

Dr. Brožek, a native of Mělnik, Czechoslovakia, came to the United States in 1939. He took his Ph.D. in physiology at the University of Charles in 1937, where he also taught for a year. Later he was an industrial psychologist with the famous Bata Shoe Factories, and since 1941 has been at the University of Minnesota's Laboratory of Physiological Hygiene.

THE deterioration of the over-all performance capacity in the presence of normal aging is well established. There are decreased motor speed, strength, and coordination, impairment of sensory and mental functions, and changes in personality.

In a number of psychological functions the decline in fitness is already evident in the thirties, becomes marked in the fifties, and is striking in the seventies.

One of the sensory functions investigated in our laboratory was the efficiency of the light-registering apparatus, measured by the flicker test. When we look at an interrupted light, we perceive it as a flicker, first as a coarse flicker, later—when the rapidity of the light flashes per second increases—as a fine flicker, and, finally, as a fused light. The greater the frequency at the fusion level (FFF), the greater the sensitivity (efficiency) of the retinocortical system. The measurements were made on women (Table 1). The decrease in this function with age was observed by several other investigators. Some sensory functions—e.g., color vision—show little or no deterioration with age.

TABLE 1

Age	N	FFF	S. D.
18–26	19	46.7	4.2
26–35	17	45.7	3.6
36–45	8	45.4	2.9
46–60	12	40.9	3.3

There have been a number of good investigations on changes in the capacity for *motor,* especially manual, performance. W. R. Miles[1] measured the speed of performance of the hand in a reaching-and-placing test. The test score in this simple manual task represented the *time* required for the completion of a short series of movements. In men there was very little change between 20 and 50, in women between 20 and 45, years of age. There was a progressive deterioration of performance in the higher age groups. Using the average time-score at 20–24 as 100 per cent, we obtain, for men:

Age	30	40	50	55	60	70	75	80
%	103	104	105	112	122	125	143	167

K. R. Smith[2] studied an operation which was closer to an industrial task, namely, the assembly of nuts and bolts. The men worked without interruption for four hours at a stretch. The age decrement in the ability to perform a high-speed manual task was marked. If we designate the value of the performance—number of assembled nut and bolt units—for the age group of 30 years as 100 per cent, the mean value decreases to 93.6 per cent for the 40-year group and to 86.4 per cent for the 50-year group. Statistically, the difference between the mean score at 30 and at 50 was highly significant. One important fact, however, should not be neglected. These are *averages.* There are some individuals at 50 who do considerably better than the average 30-year-old man.

And there is another point, demonstrated experimentally by W. R. Miles and C. C. Miles[3]—namely, that frequently experience, the continued exercise of a function, can act as a factor delaying the decline of manual skill. These investigators have administered a cube assembly test to a group of adults 20 to 90 years of age. As one would expect on the basis of other data, there was a gradual decline in manual dexterity, as measured by this test, up to the sixties, and there was a larger decrement from then on. As before, this was true of the group averages. However, and this is the point I want to bring out here, those older men who were engaged in jobs which involved rapid manipulation of parts retained the speed rate of young adults into late maturity.

In the studies by De Silva[4] on age and safety of driving, it was demonstrated that some of the physiologically relevant components of driving fitness—such as glare vision, steering (eye–hand coordination), and braking reaction—show a steady deterioration with age, starting in the thirties. Nevertheless, the safety of driving *increases* up to 50 years of age; it declines rapidly in the next two decades. The improvement in safety of driving, in spite of the deterioration in the bodily machinery, indicates that safe driving depends, importantly, on other factors, such as the generally preferred speed, taking chances, and showing off. Here we deal with personality characteristics rather than with the physiological limits of performance.

Researches on the effects of aging on *mental* abilities have firmly established that the over-all pattern of changes is similar to that of the sensory and motor functions.

The over-all pattern of changes with age is similar to the sensory and mental functions. It should be noted, again, that not all intellective functions follow the same course of decline with age. These differential trends may be explained, in part at least, in terms of practice, of use and disuse. Abilities exercised during our adult years (such as maintenance of the vocabulary, the capacity to understand and use different words), do not decline with adult age and may even increase. In performing tasks that are remote from daily life, such as thinking in terms of geometrical symbols, there is a definite and marked decrement with age.

Foulds and Raven[5] studied normal changes in the mental abilities with age in a group of Scotch industrial employees. The group consisted largely of skilled workmen; 8 per cent of the sample consisted of administrative officers; there were few unskilled workmen and 5 per cent were young people in training. The ages were 15 to 65. Two group tests were used: a vocabulary test, as a measure of the general fund of information; and a nonverbal test of a person's capacity to make comparisons, to reason by analogy, and to apply logical methods of analysis. No time limits were imposed. The total sample included about 2,000 men. Using the 20-year-old group as the reference point (their median = 50), the following median percentile values were obtained at succeeding age levels:

Age	20	30	40	50	60
Test 1	50	58	58	57	54
Test 2	50	48	43	40	38

The figures indicate that the average person's ability to recall information increases to about 25 or 30 years and remains largely constant for the next three decades. This was true for the group as a whole, with some decline in the sixties. It is of interest to note that, in people of above-average intellectual capacity, the ability to recall information apparently continues to increase, slowly, even up to the age of sixty.

The other function studied by Foulds and Raven[5]

behaved quite differently. The capacity to form comparisons and reason by analogy declines progressively with age, with the median performance at 50 years of age comparable to the fortieth percentile of the 20-year-old men. If the decline should proceed at a similar rate after the age of 60, it appears that by the age of 80 the average person's capacity to grasp ideas and reason by analogy would be not greater than that of an 8-year-old child.

We are not really concerned here with the 80-year-olds. We are not concerned with the psychology of senility but with the psychology of later maturity, perhaps the ages between 30 and 60. The investigations carried out by Foulds and Raven are in general agreement with the findings of other investigators. They indicate that from about the age of 30 years onward a person's ability to understand new ideas, to adopt new methods of working, and even to adjust to new situations steadily decreases. On the other hand, he is normally able to recall *acquired* information long after his capacity to adapt—intellectually and socially—to new situations has begun to decline.

Age and mental ability in intellectually superior men were studied by Sward.[6] His subjects were ninety members of two universities in California. The forty-five younger men were 25 to 35 years old (mean 31); the minimal age for inclusion in the older group was 60 years (range 60–79, mean 66, median 65). The subjects were matched by professional groups; e.g., an older chemist was paired with a younger man in the physical sciences. The test battery included eight subtests involving such tasks as arithmetical reasoning, use of codes (substitution of symbols for numbers, translation of a passage in English into a novel code language), and indication of the meanings of words.

The total mean score was 428 ± 29 points in the younger and 373 ± 46 points in the older group. The difference was in the same direction for all the subtests but one, the test of "Synonyms and Antonyms," involving comparison of two words at a time, in which the older men excelled the younger ones. They did less well on the "Word Meanings" test, but the difference was not statistically significant. The greatest difference was obtained in the tests involving the use of codes ("Artificial Language" and "Symbol–Digit Substitution"). These were tests with rigorous time limits and required formation of a new work method. On both scores the older men were definitely handicapped. The retardation, the slowing down of the mental processes, was apparent also on those tests that had no prescribed time limit. For the "Ingenuity Problems," a test of capacity for problem-solving, the younger men took on the average 56 minutes, the older men 77 minutes. The total test time was 3 hours and 3 minutes, and 3 hours and 53 minutes.

The means indicate average trends. It should be noted that in this study, as in all others without exception, the individual differences *within* each group are far more impressive than the differences *between* the two age groups. The impairment of the higher mental processes is by no means an invariable concomitant of chronological aging. In Sward's study there were bright and alert "senescent" subjects, subjects beyond 60, who showed few or no unambiguous signs of any decided psychological decline.

When we come to matters of *personality* and aging, our data become much less plentiful than in reference to the sensory, motor, and intellective characteristics. In part, in large part, this is due to the fact that our techniques of measurement are recent and not fully satisfactory. In talking on aging and personality, especially the personality of the research worker, we are largely speculating. But perhaps such a speculation will not be fruitless if some of the problems emerge in a sharper outline.

What is it that constitutes the "drive" in a research worker? Why do two people of the same intellectual horsepower and the same technical research possibility differ widely in their research output? How does the character of the "drive" change throughout the years? We know little of the effects of the perceived changes in one's physiology—decreased speed and strength—on one's personality. How does the concept of oneself change during the years? How does one's time perspective change?

The external incentives to work reach their peak not beyond the age of 50—incentives as measured in terms of financial reward, guaranteed job, etc. But frequently we find these people working harder than ever. Is it perhaps a compensatory phenomenon, a reaction to the fear of possible decline in performance capacity?

Let us consider, at this point, not the psychological characteristics of the research worker but the accomplishment. I need not stress that an evaluation of the creativity and productivity of a research worker is a difficult task. This is true not only of the man who works in industry and is engaged in developmental research, but also of the academic man who deposits the results of his labors in journal articles and in books.

In scientific work, one of the criteria of extraordinary achievement is the starring in the volume entitled *American Men of Science*. S. S. Visher[7] studied the age at which the representatives of

various sciences were starred (i.e., the first time). In 1943, the median age of those starred for the first time varied from 36 in astronomy and 37 in mathematics to 50 in physiology and 52 in pathology. The average for the eleven disciplines tabulated was 44 years.

Much attention was devoted to the question of age and creativity by Harvey C. Lehman. One point that he investigated was the chronological age at which prominent scientists and creative workers produced their masterwork.[8] Two points may be determined from a glance at his graphs: In *no* field is the creative capacity limited to a narrow age range. In most fields it extends from 25 to 75; only rarely it goes below 20 years (e.g., in mathematics). Second, the modal year of top achievement falls, without exception, between 30 and 35 years. Of course the distributions are skewed toward the higher age levels and the means are higher than the modal values.

A somewhat similar attempt to evaluate the age at which scientists do their best work was made by C. W. Adams.[9] His list contained more than 4,000 scientists whose ages at the time of their chief work could be reasonably accurately determined. The median value is 43 years; 9 per cent of the sample were below 30 years. It is of some interest to look at the several centuries covered by the author. The data are surprising: The median age at which the best work was done was 42 years for the men of the seventeenth century, 42 years for the men in the eighteenth century, 42 in the nineteenth century, and 44 in the twentieth century; the latter value is based on a sample of men who died in the period 1933–44. The stability of the median figures is truly surprising. But let us be clear about it: The values indicate that in 50 per cent of the cases the best work is accomplished in the forties and later.

How about the prime of men working in different fields? Table 2 gives the median figures.

TABLE 2

Field	Age	Field	Age
Mathematics	37	Astronomy	45
Bacteriology	38	Surgery	45
Chemistry	38	Psychology	45
Physiology	40	Geology	46
Physics	40	Botany	46
Engineering	43	Zoology	46
Pathology	44	Anthropology	47

In conclusion, then, psychological capacities increase during childhood, remain a plateau for a varying length of time during the adult life, and then begin to decline. I wish to emphasize strongly that there is a wide range of variation in the sensory, motor, intellectual, and personality characteristics within every age group. In different individuals the aging process proceeds at different rates. In a less dry fashion this thought was expressed by F. D. Drewitt (cited by C. W. Adams[9]). He said: "Humans vary, as apples do, some ripen in July, others in October."

Our information on the changes with age is pretty good as far as sensory capacities, motor functions, and abstract intelligence are concerned. Our understanding of personality dynamics, of the factors that move and drive, is very inadequate. Successful research does not depend on manual dexterity, nor does it require—in a large percentage of research jobs—a remarkably insightful and original mind. It depends, to a large extent, on good judgment exercised with reference to selection of the problem and of the attack, and on experience in the execution of the details, in the organization of the research operations. The work habits—taking notes, keeping abreast of literature in the field of specialization—are not as vulnerable to age as visual acuity or physical endurance.

I have brought up the importance of the changing time perspective of the aging individual. Someone has said that "Research is one function of business which, far more than any other, deals with the future." An older man—unconsciously—may be tempted to reject long-range projects of which he personally is not likely to see the outcome, the completion of which is beyond his lifetime schedule.

The bright, unorthodox, new ideas are likely to come from the younger man. But perhaps we should not forget the line of the French poet Alfred de Vigny, emphasizing that the fruition of an idea frequently takes a lifetime. In De Vigny's words, "Genius is a thought of youth realized during maturity" (*Le génie est une pensée de jeunesse réalisée dans l'âge mûr*).

One final point. The character of research is changing, with more emphasis on teamwork. Sir Heilbron (cited by D. H. Killefer,[10] in *The Genius of Industrial Research*) stated aptly: "Gone are the days of Priestley and Faraday, who with little or no assistance could carry out highly significant experiments. Almost all of our major chemical problems now require the coordinated efforts of research teams, in which each individual forges an important link." This increases the potential usefulness of the older research worker. Because of his experience in research administration, planning, and evaluation he may be increasingly useful even after his capacities for original and creative endeavor have begun to decline.

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Sociological Viewpoint

EWAN CLAGUE

Dr. Clague, Commissioner of Labor Statistics, U. S. Department of Labor, since 1946, was formerly with the Social Security Board. He took his Ph.D. at the University of Wisconsin in 1929, where he was an instructor from 1921 to 1926. He has since worked with the Metropolitan Life Insurance Company, Yale's Institute of Human Relations, and Pennsylvania School of Social Work.

OURS is an aging population. The advances of medical science have enabled a growing proportion of the population to survive to old age (Table 1).

TABLE 1*

	45–64 Years	65 Years and Over
1900	10½ million (about), or 1 in 7	3 million, or 1 in 25
1950	31 million, or 1 in 5	11½ million, or 1 in 13
1980	43 million	22 million

* Source: *Fact Book on the Employment Problems of Older Workers.* Washington D. C.: U. S. Department of Labor, Bureau of Labor Statistics (1950).

The length of the working life has also increased. In 1890, about one fourth of the working population was 45 years or older. Today this age group constitutes over a third of the labor force. For the average man, however, working life has shortened as life expectancy has lengthened. The proportion of men 65 years and over in the labor force is much smaller now (1950) than in 1900—45 per cent in contrast to 63 per cent.

Pension programs make it possible for some men to retire who would otherwise be forced to work. Very many more are forced out of the labor market by the rigid requirements of modern industry, with its emphasis on speed and dexterity, and its dilution of skills.

Census figures suggest, moreover, that white-collar and professional and semiprofessional workers are as much subject to the age requirements of modern industry as any other group of employees. Smaller proportions of these groups than of the employed population are between 45 and 65 years of age and over 65, as shown by figures for April 1, 1948 (Table 2). These are broad categories, of course, and cover many different occupational groups and skill levels. Detailed information about

TABLE 2

Occupational Group	Percentage of Employed Persons Who Were	
	45–64 Years	65 Years and Over
All occupations	30.3	4.7
Professional and semiprofessional	28.7	4.2
Clerical, sales, and kindred workers	22.8	2.0
Clerical, sales, and kindred workers	35.7	3.8
Operatives and kindred workers	24.1	2.3
Service workers	37.2	7.7

the aging problem in the host of occupations in our economy is yet to be gathered.

The Bureau of Labor Statistics has just completed a study of the employment, education, and earnings of leading scientists included in the 1949 edition of the directory *American Men of Science.** This study is the first comprehensive analysis that has been made of the characteristics of our leading scientists. About two thirds of the 41,737 scientists in the survey held Ph.D. degrees (Table 3). At the time of the survey (mid-1948), over 40 per cent of them were educators. Over three fourths had been or were engaged in research. Information on

* *Employment, Education and Earnings of American Men of Science.* Washingon, D. C.: U. S. Department of Labor, Bureau of Labor Statistics (in press).

TABLE 3

Percentage of American Men of Science in Each Age Group by General Field of Specialization and Highest Degree Earned

Highest Degree Earned by General Specialty	Total Reporting Age		Age Bracket									
	Number	Per cent	Under 30	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70 and over
All fields	41,384*	100.0	6.3	16.9	17.3	16.7	13.1	10.5	8.4	5.8	3.2	1.8
Bachelor's or no degree	4,752	100.0	11.6	15.5	13.7	12.9	11.5	10.2	10.2	7.1	4.4	2.9
Master's degree	6,315	100.0	7.0	15.0	14.5	13.5	13.4	11.6	11.0	7.9	4.1	2.0
M.D. "	4,427	100.0	1.9	8.9	14.0	15.9	14.9	14.6	12.0	9.1	5.0	3.7
Ph.D. "	25,815	100.0	5.9	19.0	19.3	18.3	13.1	9.6	6.8	4.5	2.3	1.2
Other degree and no report	75	—	—	—	—	—	—	—	—	—	—	—
Chemistry	10,547	100.0	10.5	24.0	19.5	15.5	10.5	8.5	5.4	3.4	1.8	.9
Bachelor's or no degree	1,044	100.0	16.3	19.0	15.7	12.5	10.5	10.2	7.2	4.3	2.0	2.3
Master's degree	1,098	100.0	9.7	22.3	17.8	12.9	8.6	9.0	8.6	6.1	3.6	1.4
M.D. "	133	100.0	6.8	20.3	27.1	12.8	10.5	8.3	6.0	3.0	3.0	2.2
Ph.D. "	8,265	100.0	9.9	24.9	20.1	16.3	10.8	8.2	4.8	2.9	1.5	.6
Other degree and no report	7	—	—	—	—	—	—	—	—	—	—	—
Engineering	4,861	100.0	6.5	15.2	15.6	15.5	13.1	11.1	9.2	6.7	4.4	2.7
Bachelor's or no degree	1,618	100.0	9.0	14.6	12.9	12.9	12.6	10.6	10.7	7.8	5.3	3.6
Master's degree	1,599	100.0	5.5	11.4	13.3	14.6	14.3	14.1	10.7	8.3	5.4	2.4
M.D. "	6	100.0	16.7	—	—	—	16.7	16.7	—	—	49.9	—
Ph.D. "	1,622	100.0	5.1	19.6	20.6	19.4	12.4	8.6	6.4	3.9	2.1	1.9
Other degree and no report	16	—	—	—	—	—	—	—	—	—	—	—
Physics and electronics	3,413	100.0	10.6	21.0	18.5	16.2	12.0	8.0	5.6	4.0	2.6	1.5
Bachelor's or no degree	361	100.0	28.8	20.8	15.2	11.6	8.0	5.3	4.2	3.0	1.7	1.4
Master's degree	617	100.0	13.6	21.7	15.9	13.8	10.5	8.8	7.6	4.4	2.4	1.3
M.D. "	9	100.0	—	44.5	—	11.1	22.2	11.1	—	—	11.1	—
Ph.D. "	2,420	100.0	7.1	20.8	19.8	17.5	12.9	8.2	5.4	4.1	2.7	1.5
Other degree and no report	6	—	—	—	—	—	—	—	—	—	—	—
Earth sciences	2,070	100.0	3.9	15.0	16.9	19.7	13.4	10.8	8.5	5.6	4.2	2.0
Bachelor's or no degree	293	100.0	8.9	17.1	13.0	19.1	8.5	9.6	6.8	8.9	6.1	2.0
Master's degree	453	100.0	8.4	22.1	16.8	13.7	11.0	9.7	7.5	5.3	3.1	2.4
M.D. "	2	100.0	—	—	—	—	—	—	50.0	—	—	50.0
Ph.D. "	1,317	100.0	1.3	12.1	17.8	22.1	15.3	11.5	9.1	4.9	4.1	1.8
Other degree and no report	5	—	—	—	—	—	—	—	—	—	—	—
Agriculture	2,409	100.0	2.7	10.8	13.9	14.5	14.6	14.8	13.9	9.0	4.4	1.4
Bachelor's or no degree	263	100.0	2.7	6.1	9.1	9.1	10.3	14.4	23.2	14.0	8.4	2.7
Master's degree	787	100.0	2.7	7.9	9.6	11.4	15.5	15.1	16.9	12.7	6.0	2.2
M.D. "	6	100.0	—	16.7	—	16.7	16.7	16.7	—	—	33.2	—
Ph.D. "	1,347	100.0	2.8	13.4	17.3	17.4	14.9	14.6	10.4	5.9	2.5	.8
Other degree and no report	6	—	—	—	—	—	—	—	—	—	—	—
Biology	5,767	100.0	3.6	15.3	17.8	18.4	13.8	10.3	9.5	6.7	2.8	1.8
Bachelor's or no degree	337	100.0	5.0	10.4	11.3	11.6	14.5	12.8	15.7	9.8	5.3	3.6
Master's degree	715	100.0	7.5	11.8	14.1	14.8	15.8	10.9	11.8	7.3	3.6	2.4
M.D. "	332	100.0	3.3	14.8	17.5	17.5	13.0	8.4	10.8	8.4	2.1	4.2
Ph.D. "	4,368	100.0	2.9	16.3	19.0	19.6	13.5	10.2	8.5	6.2	2.4	1.4
Other degree and no report	15	—	—	—	—	—	—	—	—	—	—	—
Medicine	2,831	100.0	1.2	6.1	12.7	15.3	15.9	16.8	13.3	9.5	5.6	3.6
Bachelor's or no degree	27	100.0	—	11.1	11.1	14.8	11.1	22.3	14.8	7.4	3.7	3.7
Master's degree	30	100.0	3.3	10.0	6.7	10.0	23.3	10.0	33.4	3.3	—	—
M.D. "	2,579	100.0	1.2	6.0	12.5	15.2	15.3	16.8	13.4	10.0	5.8	3.8
Ph.D. "	192	100.0	1.0	6.2	16.2	18.8	23.4	16.7	8.9	4.7	3.6	.5
Other degree and no report	3	—	—	—	—	—	—	—	—	—	—	—
Fields related to medicine	2,644	100.0	3.1	14.1	18.1	19.3	14.2	10.5	9.2	6.3	3.2	2.0
Bachelor's or no degree	73	100.0	9.6	21.9	12.3	8.2	12.3	9.6	2.8	11.0	8.2	4.1
Master's degree	102	100.0	4.9	15.7	13.7	14.7	14.7	7.8	19.6	5.9	2.0	1.0
M.D. "	1,236	100.0	2.4	11.7	15.1	17.4	14.3	12.7	10.4	8.5	4.3	3.2
Ph.D. "	1,232	100.0	3.2	15.9	21.9	22.1	14.3	8.5	7.4	3.9	2.0	.8
Other degree and no report	1	—	—	—	—	—	—	—	—	—	—	—
Mathematics and statistics	1,913	100.0	5.8	14.6	15.9	16.3	14.7	10.1	8.6	7.3	4.8	1.9
Bachelor's or no degree	66	100.0	21.2	9.1	13.6	4.6	7.6	10.6	16.6	4.6	9.1	3.0
Master's degree	246	100.0	5.3	9.8	13.8	8.1	13.0	13.4	11.8	14.6	6.9	3.3
M.D. "	6	100.0	—	—	—	33.3	16.7	16.7	33.3	—	—	—
Ph.D. "	1,594	100.0	5.3	15.6	16.4	18.0	15.3	9.5	7.7	6.3	4.3	1.6
Other degree and no report	1	—	—	—	—	—	—	—	—	—	—	—

TABLE 3—*Continued*

Highest Degree Earned by General Specialty	Total Reporting Age		Age Bracket									
	Number	Per cent	Under 30	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70 and over
Psychology	1,570	100.0	5.7	15.0	16.4	17.3	14.0	10.7	9.8	6.4	2.7	2.0
Bachelor's or no degree	13	100.0	7.7	23.0	7.7	15.4	15.4	7.7	7.7	—	7.7	7.7
Master's degree	64	100.0	17.2	15.6	14.1	6.2	7.8	4.7	18.8	10.9	1.6	3.1
M.D. "	18	100.0	—	—	5.6	—	27.7	5.6	22.2	22.2	5.6	11.1
Ph.D. "	1,473	100.0	5.3	15.1	16.8	18.1	14.0	11.1	9.2	6.0	2.6	1.8
Other degree and no report	2	—	—	—	—	—	—	—	—	—	—	—
Other	3,359	100.0	4.4	14.9	18.3	18.1	15.7	10.6	8.3	5.6	2.6	1.5
Bachelor's or no degree	656	100.0	9.3	15.1	15.4	14.8	12.6	8.8	11.0	7.1	3.5	2.4
Master's degree	604	100.0	3.2	14.2	16.7	14.9	18.7	11.6	9.6	7.8	1.8	1.5
M.D. "	100	100.0	3.0	13.0	15.0	19.0	23.0	11.0	5.0	5.0	3.0	3.0
Ph.D. "	1,986	100.0	3.2	15.2	20.0	20.1	15.4	10.7	7.2	4.5	2.5	1.2
Other degree and no report	13	—	—	—	—	—	—	—	—	—	—	—

• Does not include 353 scientists not reporting age.

the older American Men of Science who are still active in their professions, therefore, can tell us something about a group of people regarding whom we have as yet extremely little information—the older research workers.

American Men of Science tend to be older than all employed men.† The proportion of these scientists who were 45–64 years was 38 per cent (mid-1948), as opposed to a figure of 30 per cent for the entire male labor force (April 1950). Similarly, 6 per cent of these scientists had passed their 65th birthday, as compared to 5 per cent of the nation's labor force. The American Men of Science also tended to be older than all other members of their respective professions taken together.

Among American Men of Science, it was the younger men who were more likely to be employed in private industry. With the increase in age there was a slight increase in the proportion employed by educational institutions and a sharp decrease in the proportion employed in private industry. Whereas 33 per cent of the men under 45 years were working for business firms, only 14 per cent of those 65 or over were so employed (Table 4). From another point of view, 7 per cent of the scientists employed exclusively by educational institutions had passed their sixty-fifth birthday, compared with 3 per cent of those in private industry. The relative youthfulness of the American Men of Science in private industry is undoubtedly due in part to the increasing utilization of Ph.D. scientists in this sphere of activity in recent years.‡ But the higher proportion of older men in education may also indicate a greater acceptance of the older employee on the campus than in business firms.

It is interesting to observe the increase in the proportion of independent consultants among the older men. About one in ten of the group 65 years and over, as opposed to less than 1 in 50 of those under 65, was employed exclusively in such work. Among the engineers, one out of four independent consultants was 65 or older. Consultation work offers real advantages to older scientists. It enables them to adjust their workload to suit their own needs, at the same time permitting them to utilize their accumulated knowledge and experience. The tendency toward an increase in self-employment among older workers is characteristic of the population as a whole. Both for scientists and for the entire labor force, however, the tendency toward self-employment is in some measure the result of curtailed opportunities for salaried employment at advanced ages.

There are indications that the "aging" process begins at an older age among research scientists than among the population as a whole. Industrial workers over 45 often have difficulty finding new jobs. In the postwar period, men over 45 have formed the largest group of those who exhausted their unemployment benefits. During the depression, men in their fifties often dropped out of the labor market after years of fruitless job-seeking.

Among American Men of Science, there was practically no unemployment at any age level. Moreover, the highest median salaries were received by the scientists between 55 and 60 years of age (Table 5). Median earnings tended to increase with age up to 55–59 years. The middle salary in this age group was $6,670 a year, as compared to $5,850 for all scientists. Moreover, median earnings declined only slightly through 65–69 years. The scientists between 65 and 70, with a median salary

† Six per cent of American Men of Science were women.

‡ Since two thirds of the scientists included in the survey held Ph.D. degrees, all the figures for scientists are greatly influenced by the characteristics of the Ph.D.s. The Ph.D.s were generally a young group; about two thirds of them were under 45 years of age.

TABLE 4

TYPE OF EMPLOYER, BY AGE, OF AMERICAN MEN OF SCIENCE

Age Group	Total	Education	Government	Private Industry	Foundation	Consultant	Education and Government	Education and Consultant	All Other
Under 45 years	100.0	35.2	13.1	33.0	2.5	0.5	2.0	6.4	7.3
45–64 years	100.0	40.0	14.6	19.9	1.6	1.8	3.1	7.5	11.5
65 years and over	100.0	41.4	10.4	13.6	3.1	8.8	2.5	5.9	14.3

of $6,350, still tended to earn more than the group between 40 and 45, who received a median salary of $6,250 a year. Scientists 70 years or older had a middle salary of $5,650 a year.

The tendency for the older American Men of Science to earn more than the younger ones, and for the highest median salaries to be received by the scientists in their fifties, was true for those at every level of education (bachelor's, master's and doctor's degrees). In no case did median annual salaries decline appreciably among the scientists past 60 years of age.

Among the Ph.D.s, who constitute two thirds of American Men of Science, the tendency for salaries to increase with age was true for every specialty. But the rate of increase and the age at which peak median salaries were received varied from one specialty to another. In two such widely disparate

TABLE 5

MEDIAN AND QUARTILE SALARIES AND INTERQUARTILE RANGE OF SALARIES BY LEVEL OF EDUCATION AND AGE (FROM *American Men of Science*)

Level of Education	Total*	Regular Annual Salary of Scientists According to Age									
		Under 30	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70 and over
All scientists†											
Upper quartile	$7,600	$5,350	$6,420	$7,400	$8,020	$8,500	$8,800	$8,900	$8,700	$8,480	$8,440
Median	5,850	4,440	5,260	5,850	6,250	6,360	6,480	6,670	6,550	6,350	5,650
Lower quartile	4,610	3,600	4,290	4,660	4,920	4,960	5,060	5,160	5,090	5,010	3,620
Interquartile range	2,990	1,750	2,130	2,740	3,100	3,540	3,740	3,740	3,610	3,470	4,820
Ph.D. degree											
Upper quartile	7,200	5,500	6,440	7,210	7,670	7,850	7,980	8,050	8,050	7,720	7,500
Median	5,720	4,580	5,280	5,730	6,000	6,070	6,170	6,300	6,290	6,070	5,520
Lower quartile	4,580	3,720	4,310	4,600	4,820	4,860	4,980	4,990	5,050	4,980	3,610
Interquartile range	2,620	1,780	2,130	2,610	2,850	2,990	3,000	3,060	3,000	2,740	3,890
Master's degree											
Upper quartile	7,230	4,930	6,180	6,910	7,730	7,960	7,900	8,370	8,100	7,850	6,670
Median	5,610	4,120	5,120	5,630	5,930	5,940	5,920	6,350	6,020	6,160	4,460
Lower quartile	4,420	3,270	4,180	4,550	4,630	4,720	4,710	4,930	4,650	4,690	3,270
Interquartile range	2,810	1,660	2,000	2,360	3,100	3,240	3,190	3,440	3,450	3,160	3,400
Bachelor's or no degree											
Upper quartile	8,680	5,100	6,490	7,960	9,260	10,000+	10,000+	10,000+	9,970	10,000+	9,670
Median	6,450	4,390	5,320	6,490	7,280	7,780	7,910	8,090	7,430	7,160	6,500
Lower quartile	4,910	3,540	4,430	5,240	5,890	5,960	5,870	6,290	5,780	5,460	4,000
Interquartile range	3,770	1,560	2,060	2,720	3,370	—	—	—	4,190	—	5,670
M.D. degree											
Upper quartile	10,000	5,250	6,620	9,060	10,000+	10,000+	10,000+	10,000+	10,000+	10,000+	10,000+
Median	7,430	4,110	5,170	6,900	7,830	8,390	8,920	8,150	8,310	8,140	7,330
Lower quartile	5,210	3,330	3,800	5,250	5,860	5,990	6,190	6,100	6,030	5,360	4,330
Interquartile range	—	1,920	2,820	3,810	—	—	—	—	—	—	—

* The upper quartile, median, and lower quartile in every case is based on the total number at each level of education, including a few in each case who did not report age.

† Based on all scientists, including 15 scientists holding degrees other than those specified, and 63 who did not report level of education.

fields as engineering (the highest-paid specialty of American Men of Science) and biology (the lowest-paid field), it was the group between 60 and 65 who had the highest salaries (Table 6). This was also true in fields related to medicine, agriculture, and psychology. In physics and electronics, however, the scientists between 40 and 45 years of age had the highest salaries. This does not mean that tended to rise more sharply with age in private industry than in any other type of employment. Half the scientists 55–65 years of age who were employed by business firms earned $10,000 a year or more.

It appears, therefore, that unlike industrial workers, who begin to face the problem of aging in their forties, these scientists were able, on the average, to continue to increase their earnings through their fifties. Of course these figures are

TABLE 6

MEDIAN SALARIES OF AMERICAN MEN OF SCIENCE WITH PH.D.S, BY FIELD OF SPECIALIZATION AND AGE

SPECIALTY	TOTAL*	ANNUAL SALARY ACCORDING TO AGE									
		Under 30	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70 and over
Chemistry	6,030	4,830	5,790	6,370	6,720	6,460	6,580	6,860	6,500	6,110	5,500
Engineering	6,960	5,130	6,360	7,120	7,460	7,570	8,210	7,810	9,170	7,500	5,000
Physics and electronics	5,960	4,450	5,520	6,440	6,690	6,510	5,940	5,880	5,880	5,820	†
Earth sciences	5,710	†	4,730	5,290	5,720	6,110	6,220	6,750	6,500	6,600	†
Agriculture	5,660	4,560	5,120	5,230	5,700	5,970	6,200	6,520	6,790	5,810	†
Biology	4,940	4,070	4,430	4,720	5,080	5,290	5,550	5,700	5,910	5,630	5,400
Medicine	5,830	†	†	5,210	5,950	7,000	6,130	†	†	†	†
Fields related to medicine	5,420	3,900	4,720	5,250	5,490	5,840	6,280	6,470	6,640	6,290	†
Mathematics and statistics	5,060	3,770	4,460	4,950	5,210	5,460	5,720	5,720	5,500	5,890	†
Psychology	5,320	4,020	4,670	5,420	5,640	5,670	5,450	5,410	5,820	5,750	†
Other	6,520	5,070	5,460	6,320	6,950	6,960	7,350	7,080	7,360	7,750	†
All scientists	$5,720	$4,580	$5,280	$5,730	$6,000	$6,070	$6,170	$6,300	$6,290	$6,070	$5,520

* Includes those scientists who did not report age.
† Median salary not computed, since fewer than 20 scientists reported.

physicists, unlike other scientists, begin to decline in productive capacity and earning power at a relatively young age. Rather, these salary figures reflect, in great measure, the unprecedented demand for nuclear physicists and electronic scientists in recent years, a demand which has tended to outrun the available supply and had pushed salaries in these fields above previously existing levels. At the same time, it was the younger men, for the most part, who were trained in nuclear physics and who thus obtained an advantage over their older colleagues.

Not only were highest median salaries received at the older age levels by government employees and educators, who are frequently sheltered from competition by tenure rights, but also by scientists employed in private industry. In fact, salaries averages of salaries in a given year and by no means indicate that individual salaries always increase with age to the late fifties. Nor do higher salaries necessarily indicate higher productive capacities. The tendency in educational institutions and government agencies to credit employees for their length of experience on the job may very well result in an individual's receiving his highest salary after he has passed his prime. The data on the earnings of American Men of Science suggest, however, that by and large it is not before the sixties that professional ability and productive capacity begin to decline among highly trained scientists. For such people the maturity of judgment and the broad frame of reference that experience gives apparently outweigh the increasing inflexibility of advancing years.

Viewpoint of the Research Administrator

RAYMOND STEVENS

The author is well qualified to discuss the problems of age in industrial research personnel. A vice president of Arthur D. Little, Inc., he has been engaged in industrial research since 1920, and for a good portion of that time has been concerned with research administration. In 1940 he directed a national survey of research in industry for the National Research Council and the National Academy of Sciences.

A RECENT magazine article asserted that the day of the older man is here. It noted the current worship of the romantic older man by both bobby-soxers and their elder sisters, citing the obvious attraction Ezio Pinza exerted in *South Pacific* as but one example among many in the entertainment and fiction fields. Perhaps, said the writer wistfully, we are coming to appreciate the worth of the man with the graying temples, the man of maturity, perhaps we shall even admit that a grandfather can be glamorous. Frivolous as this passing reference may be, there is no doubt that older men will be "younger" as the years go by and the science of geriatrics develops means to make them so. Americans live longer today than their great-grandfathers; they are likely to remain vigorous, healthy, and active for a far longer period in their lives; and it will soon be imperative either to set an arbitrary retirement age to remove them from top echelons in industry and society or develop a whole new era and body of workers and enjoyers—all over 60.

My concern, however, is with the particular value of the older man and the younger man in industrial research, with their integration into an industrial research organization in such a manner that the organization can utilize to the full the peculiar talents and characteristics of each. We are leaving out of consideration entirely those men who with astounding energy apply their whole efforts to invention from the age of 8 to 80. They are, we must admit, the outstanding exceptions to general rules we may lay down or conclusions we may draw —as genius is in any other sphere in which men work and create.

Can we discover pertinent facts about the relation of age to creativity—and to the management of an industrial research laboratory this is a consideration of primary importance—from past and current research on the subject? Can we apply the results of that research to a particular organization? Can we draw conclusions that will remain valid when applied to general industrial research activity? I believe we can, although the whole subject is still under study and a matter for individual interpretation in practical application.

In Sir William Osler's farewell address at Johns Hopkins he stated very bluntly that he had

> . . . two fixed ideas well known to my friends, harmless obsessions with which I sometimes bore them, but which have a direct bearing on this important problem. The first is the comparative uselessness of men over forty years of age. This may seem shocking, and yet, read aright, the world's history bears out the statement. Take the sum of human achievement in action, in science, in art, in literature,—subtract the work of the men above forty, and while we should miss great treasures, even priceless treasures, we would practically be where we are today. . . .

Osler was a man of science and a creative worker. He judged his fellow-men by his own exacting and narrow standards of creative productivity. Unfortunately, on that occasion, Osler went on to state his theory that all men over 60 might be better chloroformed, the public press took up his remarks out of context, and the whole pack of public opinion was in full cry against him. In projecting an "oslerization" of the world around him, however, Sir William was speaking specifically of creative workers. Most of us know what a creative worker is and that he is usually to be found in the fields of literature, painting, sculpture, philosophy, and now science—for no one will deny to the scientific worker the right to be considered creative when he makes signal advances in the theory or practice of his art. We can, then, take it for granted that as a creative worker the researcher in science will fall under many of the general rules for the type. He presents, however, a problem peculiar to the management of a research organization or to the executive of the research department of a company. In selecting and directing personnel, not the least of research management's problems is that of supplying the organization with a constant stream

of dynamic, inventive, creative scientists. He will find he must "catch them young." Much of the great work in science, many of the most inspiring ideas and advances, were a triumph of youth, whether one take Laplace at 20 publishing researches on the calculus and the theory of probability, Pascal completing his conic sections and other brilliant work in his teens, Perkin founding the coal-tar industry at 18, or Newlands demonstrating the law of octaves before he was 30. It will occur to the research director that the relation of age to creative productivity is one he must consider in view not only of these shining examples, but of more general data recently made available on the question.

Various studies have been made of the relation between age and creativity, and in recent years H. C. Lehman has published a series of articles on the records of inventors, composers, artists, poets, and other creative workers in relation to their ages. Dr. Lehman has found the pattern the same for every profession: a general peak of creative activity tends to occur at an age between 30 and 40—it varies slightly from profession to profession—and very often between 30 and 35. The frequency of creative activity as related to quality of production declines sharply after that important decade.

The weight of evidence gathered by Dr. Lehman seems to imply that the probability of new contributions is greater with the younger men, and that the period between 25 and 35 is all-important for useful ideas in research. Dr. Lehman writes that the peak of quality is reached earlier (between 30 and 40) than the peak of quantity of output, and that "it seems reasonable to conclude that the best and most brilliant creative accomplishments are the work of individuals who 'hit their stride' at not too old an age level."

A check on the invention record of our own laboratories over a period of twenty-five years found it to fall on the general curve determined by Dr. Lehman's searches, even though one might have expected substantial deviations in gauging one small organization against a whole profession. We found that most inventions at our laboratories resulting in patents issued were made by men in their thirties, and that far fewer men contributed outstanding inventions after the fortieth year.

American Chemical Society employment records show that research organizations generally seek the younger men to fill openings, or in expanding. One large company goes much farther and makes an addition of 10 per cent of the total staff each year from current graduates. Five per cent are replacements and 5 per cent for growth. The chief research executive plans to maintain this source of new and young blood even if growth ceases and transfers to other work are necessary to continue. He believes that otherwise his research department will suffer obsolescence.

Within our own laboratories the search for men for the technical staff shows a definite trend toward youth in the past decade. In 1940 23 per cent were in the age group 25–30; in 1947 36 per cent were in the age group 25–30, followed by a sharp decline through succeeding age groups to a comparative low of 6.3 per cent for the age group 43–48 and only 5.4 per cent for the age group 49–54.

We believe that this search for younger scientists is directly related to the peculiar contribution they have to offer a research organization: their creativity in terms of new ideas.

The ratio of the youthfully creative to the well-tempered mind is, as I have stated elsewhere, one each research director must set up for himself. If men are generally hired at 25, and need five years of experience to develop full value, there is left a bare ten-year period between 30 and 40 for their best original creation. After that there is a period of twenty-five years to retirement, well over half of the forty years between 25 and 65. This ratio may be a normal and desirable one for the whole stabilized organization, say one third young men, two thirds men over 40. But if Dr. Lehman's findings are pertinent to the industrial research field, the important point is recognition of the different characteristics of various age groups and the use of each at their most effective levels. The orthodox procedure of subordinating youth to age in all things does not seem indicated.

No one with experience in the field of research management will tell you that the ideal total research staff consists of men between the ages of 25 and 40. Unfortunately, fewer studies appear to have been made of the correlation of age with stability, maturity of judgment, utilization of experience, dependability—factors that we tend, by simple rule of thumb, to associate with men at higher age levels. The young men are growing older in the meantime, and research management which has embarked on a policy of ensuring a production of new ideas by taking on younger staff members is faced with the twofold problem: (1) How shall a fruitful balance of older and younger age levels be established on research projects; and (2) how shall the younger men be assured of a future in which they will not bump their heads against the roof of seniority?

Our own company, dealing as it does with the

solution of a diversity of problems in applied research, has found it practicable to set up special teams for particular projects independently of organization chart, age, or previous condition of servitude. Even the very top of the organization is included in some of the project teams—and in any one instance not at all necessarily as the leader responsible for results. This policy was not recognized as a formal policy twenty years ago, but evolved slowly into an accepted arrangement because we found it worked. As we have put the cart before the horse, the practice before the theory in this case, our experience may have a bearing on studies of age and youth in industrial research. We have found that retirement of older research workers simply to clear the way for the younger is not the only means of giving younger men the freedom of action they need to use their abilities to the full.

Many executives in the research field have given thought to the problem of the older research man. Robert E. Wilson, chairman of the board of Standard Oil of Indiana, wrote last year: "It is necessary to have what might be called both a staff and a line organization in research. Some men, as they grow older in experience and possibly a little less inventive, are most useful in consulting staff positions, as associate directors, or something of that kind. Other associate directors furnish the drive, and the day-to-day direction of most of the research is put under them." One executive of a large corporation has stated to me quite frankly that his company regards the older men more valuable than young men in the development end of the company's activities. One sidelight on this problem came to my attention two years ago. It is not too pertinent and perhaps not too valid. But I found it significant in its implications.

The Holyoke (Massachusetts) Savings Bank became concerned over the increasing age of the board of trustees. Members of the bank's management drew up in cooperation with the trustees themselves a test to probe the fitness of trustees for the office they held. They felt, they said, that "even such an exceptional group of men as those who volunteer to serve our banks are subject to the same laws of human behavior as are the rest of their fellow-men." A group of trustees representing the bank's personnel committee then met and applied the special rating form to 53 trustees of three mutual savings banks of Holyoke. Their screening sheet defined physical fitness, mental alertness, open-mindedness, cooperation, and initiative as the desirable characteristics of the ideal trustee, and the team rated each man as *excellent, good, fair,* or *poor* in each of them. Of the men over 65, only 5 per cent qualified as the ideal trustee; of the group under 65, 95 per cent.

The value of older men in industrial research may vary with the profession in which they find themselves, but certain factors tend to stand fast. One well-known professor has said: "Put younger men on the impossible tasks and then give the situation to older men to develop." Dr. Shock has written that "Mental deterioration is not a necessary result of aging," and that "There is no scientific evidence for the popular dictum: 'You can't teach an old dog new tricks'." He referred to the wartime Retraining Course at Harvard Business School, where older workers were retrained for more responsible positions. "Their professors," Dr. Shock wrote, "were convinced that these older workers could retain and master new information with adequate incentives. In addition, because of their past experience in industry, they were able to contribute new ideas and procedures which were definitely original and valuable."

Perhaps the qualifying phrase used by Dr. Shock —"with adequate incentives"—goes far to mark the difference between the creativity of the younger and the older worker. We know by what changing incentives and interests we are influenced during the progress of our lives, from the ice-cream cone of the 2-year-old to the trusteeships of the man of 60.

From practical experience we all know that men's total value to industry does not necessarily diminish with age. Yet we must also recognize a change in the *character* of a man's contribution as he develops and ages. Unless we recognize and allow for those changes in character of contribution we cannot employ men most effectively or give them adequate satisfaction. Currently there is much talk about the problem of the older man in industry. The doctors have kept more of them alive. We forget that their very existence also creates a problem for younger men of ability and ambition. We forget that there is cruelty in creating frustration for young men—as much cruelty perhaps as in keeping older men at a level they can no longer match with achievement—and this may be a great social loss, the failure to use to the fullest the capacities of youth.

We must be just to older men. We must reward them for faithful past service. We must give them the security they have earned and on which they are dependent. We must use their experience and be guided by their seasoned judgment. But even at the risk of being misunderstood, and of being accused of unfairness to older men, I shall say that the problem of the younger man is not receiving its proper share of attention.

When we speak of rewarding older men for their faithful past service, should we not also consider the due compensation of the young? Why is it that a Ted Williams in his twenties receives an income based on his outstanding contribution to the game of baseball, a contribution limited by his increasing chronological age, while we give no thought to the comparable position of the young creative worker, whose particular contribution to industrial research will also be limited by the age factor? I understand that one Belgian company has started a trial of this arrangement. There are obvious income-tax and other complications in America, and it would take courage and careful planning for any one company to start such a plan, but as a speculation it has some intriguing aspects.

The problem in the industrial research laboratory of using both youth and age to their best advantage is especially acute. Only a small percentage of men in the industrial research laboratory need be creative—but that percentage is vitally important.

Scientific studies in the creative imagination are far from complete, and scanning the literature we find the majority of books written by others than scientists—by critics discussing writers and painters, or by writers discussing one another. The outstanding, the authoritative, book is the fascinating literary study of Coleridge, *The Road to Xanadu*, by John Livingston Lowes. Henri Poincaré has contributed wise and cogent essays on the subject. The influence of scientific ideas on literary imagination has been studied, attempts have been made in the medical field to associate disease, mental and physical, with the workings of genius; but nothing definitive has been established about the processes of the healthy creative scientific imagination. We have been given little guidance toward fostering its development among our young people. We now have one strong thread in our hands—the influence of chronological age on creativity.

This creative imagination needs the research man's study and, particularly, his further consideration of the age factor. We cannot expect our schools alone to develop creative imagination in students. As a nation, unfortunately, we are more prone to raise Quiz Kids than Isaac Newtons. We should not expect the feeding of facts into young people's heads to give other return than the same facts—possibly a little jumbled as the only evidence of their mental processing. Many schools turn out young men and women prepared for immediate careers, smart youngsters pat with answers, prepared to be passed from the molding pressure of school to the molding pressure of our complex industrial society. That they go from pressure to pressure is surely not conducive to creative activity as we recognize it from examples in the past.

We have always given thought in this country to the means of getting learning into people's minds. Relatively little consideration has been given to getting new thoughts out. We produce by the score people who write, but most qualified critics seem to feel that the percentage of really creative writers is too small. The record is the same in other creative areas. It is an indictment of our civilization that in the millions of tons of printed matter spewed from our presses daily there is so little evidence of creative activity. We still know nothing about the art of creating. We need ideas—creative ideas—in science as well as in the arts.

Research has become within our own lifetime complex, highly organized, and schematized. Industrial research in the past decade alone has become big business. The taxes we pay support research by government. As the acceptance of industrial research as an honored and well-paid profession sends more and more young men flocking to the laboratories, we must take increasing thought for their futures. Above all, we must concern ourselves with their successful integration into the industrial research organization.

To recapitulate: We have suggested that a division of assignment along orthodox administrative lines may not always be wise. The problems considered here involve questions not only of the organization of industrial research, but of education for it, and especially of the vital factor of the imaginative process.

Are young men getting enough attention in aging America? Budding genius needs scope; in our quite proper consideration for the grandfathers we are in danger of overlooking the young men.

In industrial research creative ideas are a must. Creative ideas are related directly to age; they come, according to research data, mainly from the young men between 25 and 35. We must do more to encourage creative thinking, even if it means paying young chemists or physicists for their years of top creativity, much as Ted Williams is paid most during his few years of greatest usefulness.

This is not the time to worry only about protecting old age. It is also not the time to concentrate solely on advancing youngsters. One way in which medical and social scientists can be of great help to management in America, and especially to the management of industrial research, is to discover and recommend ways to use men at *different* age levels most effectively and most fairly.

The Function of Trace Elements in Biology

BERT L. VALLEE

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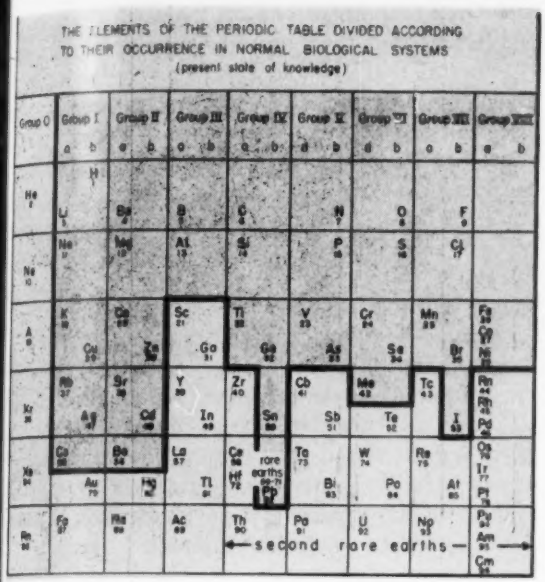
THE past twenty-five years have seen remarkable advances in our understanding of nutrition, based principally on the results of biochemical investigations of plants and animals, including man. Comparative biochemistry is a term as commonplace to the college student of today as comparative anatomy was to many of his predecessors of not long ago. Progressively, investigators have turned their attention from carbohydrates, fats, proteins, water, and salts—the more easily studied bulk of body mass—to enzymes, hormones, vitamins, and metals which, though present in minute quantities, nevertheless exert powerful influences in plants and animals alike. Without them life of any evolutionary form cannot exist. Although earlier investigators were well aware of the existence of problems involving these substances, research on them was delayed because instrumentation was either not available or not sensitive enough to deal with quantities present in concentrations as low as thousandths and millionths parts of grams.

Advancing technology has made possible an ever-widening scope for the biological investigator interested in human health and disease and the maintenance of adequate nutritional balance between man and his environment. New techniques have made possible the study of the substances present in minute quantities, with reference to their occurrence in cells and tissues and their function in living systems. These substances have been shown to be concerned with all aspects of biology. They are particularly important not only in the reactions involved in the *synthesis* of energy-rich compounds from inorganic constituents utilizing sunlight, but also in the final *breakdown* of these foodstuffs by oxidation, yielding the harnessed energy that can be utilized by another organism. The role of inorganic constituents in intermediary metabolism has long been neglected, but now the exciting story of their occurrence and function in plants and animals is beginning to unfold.

Of the ninety-eight elements of the periodic table more than sixty have been discovered in bacteria, fungi, higher plants, and animals. Few of these elements have been studied intensively. Investigation has been largely restricted to those occurring in amounts large enough to be easily measurable with available techniques. Among them we may list calcium, carbon, hydrogen, iodine, iron, magnesium, nitrogen, oxygen, phosphorus, potassium, sodium, and sulfur.

"Trace elements," "micronutrients," "microelements," "minor elements," and "oligoelements" are terms applied to the remaining elements occurring in biological systems—but in quantities so minute that no known instrumentation has allowed their measurement, and little or nothing has therefore ever been learned about their function. They have been the subject of much philosophical discussion, teleological reasoning, and controversial argumentation, all quite fruitless in view of seemingly insurmountable technical difficulties precluding any experimental settlement of the issue. As a result of this debate the term "trace element" has acquired a somewhat base and ignoble connotation, implying that these substances can only be measured qualitatively, and not quantitatively, that their presence is accidental, and therefore has no discernible consequence. This is an erroneous conclusion. Although the teleological debate may have added but little fact, it has pointed out clearly the resemblance between diseases of plants and animals due to, for example, deficiencies of copper, cobalt, manganese, molybdenum, and zinc in the



The elements above the black line have been encountered in some biological system. The concentrational occurrence and function of most are unknown.

soil, to certain pathological processes of man, the nature of which is not understood thus far. To give these elements an unprejudiced status the term "oligometals"* has been proposed to designate metallic elements occurring in the body in low concentration. In this discussion the terms "trace element" and "oligoelement" will be used interchangeably.

The oligoelements occur, and are thought to be required, in extremely small quantities, as the designations imply, and many or all of them are thought to be essential to life. The verification of this hypothesis has posed extremely difficult problems because of the inadequacy of past and present technical development. Much of the evidence has been collected with great difficulty and has not stood the test of time. Yet so much convincing material has been adduced that there can be little question that the oligoelements are important metabolites which may hold the answers to many unsolved biochemical and biological problems.

In many ways the role of trace metals in biological phenomena is not dissimilar to that of the vitamins. Metals have been found to participate in catalytic processes, either as integral parts of a protein molecule as metalloenzymes, or as "activators" of enzyme systems—in which case some observers have considered the elements involved to be coenzymes, the binding being so loose that dialysis was readily possible. In other instances, however, they have been associated with proteins serving as hormones or engaging in functions of storage or transport.

Accumulating evidence indicates an abnormal distribution of these substances in the pathological growth processes referred to collectively as cancer, in the psychoses, some neurological disorders, and in blood diseases.

The experimental advance has been consequent to major developments in techniques during the past ten years, which have finally opened new avenues of approach in the quantitative determination of very small amounts of inorganic substances. These highly sensitive techniques now reveal additional oligometals not previously known to be present. As might be expected, such advances have come from fundamental investigations in chemistry and physics, and can now be adapted for use in biology.

Biological matter more closely resembles sea water than the earth's crust in its inorganic composition, evidence of adaptation to the aqueous environment in which it originally evolved. Contingent upon their needs, different biological species have concentrated different elements from their environment to a marked degree. It has been argued that the presence of the oligoelements in higher forms is but a result of "contamination" from their food sources. Yet titanium and silicon present in high percentages in the earth's crust, upon which plants are nurturing directly, are almost completely rejected by them. On the other hand, thiospirilla and bacteria may selectively accumulate sulfur and use it in their essential energy cycle, and vanadium takes the place of iron in the respiratory pigment of ascidians, and copper similarly in the hemocyanins of crustacea. *Bertholletia excelsa*, the Brazil nut, selectively accumulates barium, and definite concentrational differences of barium are encountered between the testa and the endosperm. Brewers yeast has been found to have less aluminum, barium, cobalt, phosphorus, manganese, copper, and zinc than a comparable sample of baker's yeast, and contained boron and chromium which were absent from baker's yeast altogether. Because of the minute quantities necessary to bring about biological effects, and the lack of adequate techniques, it has been most difficult to identify the elements necessary for vital processes. Yet workers in agronomy, botany, veterinary medicine, bacteriology, and enzymology have shown conclusively that the oligoelements play an essential role in living systems, even though quantitation of these elements is still in the embryonic stage.

It is not generally known that numerous plant diseases are due either to deficiency of, or intoxication by, a metal and that many bacterial strains

* Oligo = Greek meaning "few, scanty."

are dependent on one or many elements for growth. Lack of boron, copper, manganese, molybdenum, or zinc produces well-defined deficiency diseases of great economic importance; pecan rosette of deciduous trees, canker of beets, reclamation disease, and gray speck of oats may all be cured readily by the addition of boron, copper, manganese, and zinc, respectively, to the nutrient medium (Table 1).

Since there are definite areas of soil deficiencies near the eastern and western seaboards and the Great Lakes, animal deficiency diseases caused by inadequate metal content of the fodder will occur there. Cobalt deficiency manifests itself by anemia and emaciation terminated by death. The disease is known as bush sickness, Morton Main's disease, or enzootic marasmus in different areas of the world. The addition of cobalt to the feed corrects the condition. The recent discovery proving vitamin B_{12} to be a cobalt-containing complex has stimulated interest in cobalt metabolism of man and emphasized the importance of that element in nutrition.

Similar studies have been carried out for other elements. Copper deficiency is responsible for serious and fatal diseases of cattle, goats, and sheep known as salt lick, reclamation disease, or swayback, and manganese deficiency has been found responsible for serious bone disease in chickens and pigs. On the other hand, molybdenum or selenium *intoxication* is not an uncommon cause of disease in cattle in many parts of the world.

These well-established examples demonstrate the direct relationship of certain metals to sustenance of life and that their occurrence is not fortuitous.

Experimental deficiencies in plants, microorganisms, and animals have been produced in the laboratory and have greatly enhanced our knowledge. Studies of this sort with plants and microorganisms date back to Sachs (1860), Knop and Raulin (1860), but truly successful work in man was not accomplished until the 1920s, when sudden interest on the part of many investigators produced a flood of papers which has been ever-increasing. Some workers have attempted to elucidate the problem by balance studies. Few conclusive data, however, have accumulated from this type of endeavor. Work on copper metabolism indicates that this element may have an essential role in hemoglobin formation of rodents, and its dietary deficiency, as well as those of manganese and zinc, results in inanition and death. Conclusions applicable to the physiology of man, however, are not warranted, because of the great species difference.

Recent work in Australia, England, and the United States has emphasized the important influence of molybdenum on copper metabolism in cattle. It seems apparent that delicate balances between these two and other elements exist, not easily studied by following the metabolism of only one. The ideal instrumental approach seems exemplified by emission spectrography permitting simultaneous quantitative measurement of some fifteen to twenty elements in blood and tissue.

TABLE 1 *

OLIGOMETAL DEFICIENCY DISEASES

Element	Plant Diseases	Animal Diseases	Deficient Areas
Copper (Interacts with Mo)	Exanthema, dieback, reclamation disease	Salt lick, swayback, licking disease, enzootic ataxia	Australia Denmark Great Britain Holland New Zealand South Africa South America United States
Cobalt	No pathological manifestations have been described	Pining, bush sickness, enzootic marasmus, Morton Main's disease	Australia Canada New Zealand Scotland United States
Manganese	Gray speck, pahala blight, speckled yellows, marsh spot	Laboratory-produced: Perosis of fowl and rabbits, lameness of pigs	United States
Zinc	Pecan rosette, little leaf, mottle-leaf	Failure of growth in laboratory rats and mice	United States

* From Vallee, N. N. "The Epidemiology of F, I, Mn, Cu, Co, Zn, Mo, in Soils, Plants, Animals and Man." Boston University: Thesis (1949)

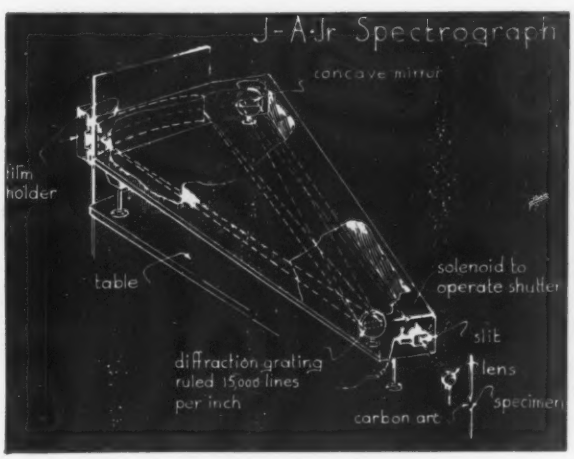
Finally, in vitro studies have scrutinized the effect of many elements on isolated enzyme and hormone systems in an effort to assign definite functional roles to these elements.

Any discussion of this subject would be inadequate without a brief review of the techniques which have made progress possible and are continuing to open new avenues of approach. These techniques are:

1. Microchemistry
2. Emission spectrography
3. Polarography
4. Ion exchange separations
5. Electron optical methods
6. X-ray methods
7. Activation analysis
8. Radioactive isotopes
9. Mass spectrography
10. Microbiological assay

Among these methods *microchemistry* stands out. Through the diligent search of various investigators, many dyes have been and are continuing to be found which selectively combine with certain metals. Some of these dyes are nonselective and interact with many metals. By adjustment of the pH of the substance to be analyzed, however, and by the addition of salts which form metal complexes, metals other than the one sought may be excluded from the reaction, and thereby specificity for one is attained. Dithizone and its derivatives are good examples of this group, though there are many others: 8-hydroxyquinoline (oxine), orthonitrosophenol, α-nitroso, β-naphthol, etc. Diphenylthiocarbazone (dithizone) combines with many elements to form metal dithizonates binding either one or two metal atoms per molecule of dye. Dithizone may be dissolved in CCl_4 or $CHCl_3$, and by separatory funnel extraction the metals in question may be extracted stoichiometrically from the aqueous solution to be analyzed. Dithizone itself is green, but the resulting zinc dithizonate is red, the cobalt dithizonate violet, the mercury compound orange, and the nickel compound brown. These compounds have characteristic spectrophotometric absorption curves and obey the Beer-Lambert law. The physicochemical properties make these dyes eminently useful from the point of view of the trace metal analyst. Microchemical quantities of the order of 1 μg can often be analyzed with great accuracy, an achievement that was impossible fifteen or even ten years ago. Since this is the order of magnitude in which some of the interesting biochemical elements may occur in blood and tissue samples available for handling, these microchemical techniques are very useful. It must be emphasized, however, that they are often complex and difficult. Because of the minute quantities involved, scrupulous cleanliness, as well as avoidance of contamination of reagents and glassware, is indispensable. And this of course applies both to colorimetric techniques and to other procedures for trace metal analysis. This point cannot be emphasized too strongly, since the problems of contamination involved are easily of the order of magnitude of those encountered in bacteriological research.

Emission spectrography utilizes the fact that elements excited by sufficient energy emit light of a characteristic spectral distribution. The simplest example of this type is the introduction of small quantities of an element into the flame of a Bunsen burner, in which case sodium, for instance, emits yellow, and potassium red, light in the visible region of the spectrum. The accepted mechanism of this effect is the excitation of atoms in the flame to higher electronic energy states, followed by emission of light when the excited atoms return to lower energy states, or when ionization takes place by complete ejection of an electron from the atom. The various transitions from higher to lower states are accompanied by the emission of light of specific wavelengths characteristic of this element. The light appears in the form of spectrum lines when viewed through a suitable prism device. Some 350,000 lines have been measured. Their interpretation is complex, though the task from the analyst's viewpoint is lessened somewhat by the fact that some lines are more persistent than others. When the concentration of an element is decreased, the number of lines observed, other factors being constant, becomes progressively fewer until only the "ultimate lines" remain; these can be relied upon to identify the presence or absence of a given element accurately.



Diagrammatic sketch of Jarrell Ash grating spectrograph.

Which electronic transitions will occur is dependent upon the electric, magnetic, and thermal influences to which the atom is subjected, as well as upon its own structure. Different forms of excitation will therefore produce varying effects. The ones in most common use are the electric arc, the spark, and the hot flame. Which one of them is most suitable for a given case is determined by experience, though as a rule the d-c arc is found most reliable and sensitive for emission spectrography of biological samples.

When the light emitted from one or several elements is introduced into the slit of a spectrograph and allowed to impinge upon a dispersing element, either a prism or a diffraction grating, the light is dispersed into its component wavelengths, which may then be recorded on a photographic film or plate where the light at each wavelength will bring about blackening in the form of a line.

This technique has the unique advantage of determining simultaneously the qualitative presence or absence of almost all elements quickly and accurately. This is true for concentrations as low in some instances as 1×10^{-8} to 1×10^{-9} g (1/100–1/1000 μg)/g of sample. This in itself makes emission spectrography an extremely valuable tool, particularly since only very small samples are necessary for useful determinations.

The technique is even more attractive when it is realized that it may be used for quantitative work quite as well. This quantitative usefulness is based on the fact that under appropriately controlled conditions the light emitted, and hence the blackening of the photographic plate, are proportional to the amounts of element present in the sample. The density of lines is therefore an index of the amount of element present, and the blackening can be measured accurately with a microdensitometer.

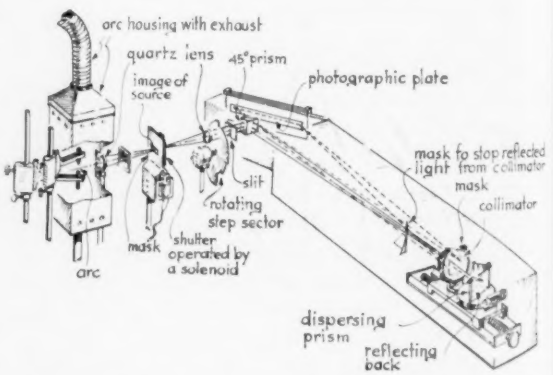
The main difficulty with which the biological investigator is confronted is the question of sample preparation. Although spectrography has been used widely in industry, it has never been adapted adequately to the needs of the biologist.

The *polarographic* method of chemical analysis was described twenty years ago by Jaroslav Heyrovsky in Prague. It is a relatively new technique on which publications in the United States did not appear until 1937. Since then the field has virtually mushroomed. The principles of this method are based on the interpretation of the current-voltage curves which are obtained when substances—which may be organic or inorganic—are electrolyzed in a cell in which one electrode, the cathode, consists of mercury, falling dropwise from a very fine bore capillary toward a stationary mercury anode. Different ions have typical conductivities, and the voltage necessary to start decomposition—the "decomposition potential"—is characteristic of the substance in question. The height of the wave, the "limiting current," is a quantitative measure of amounts present. Usually it is measured as the "half-wave" of the "limiting current."

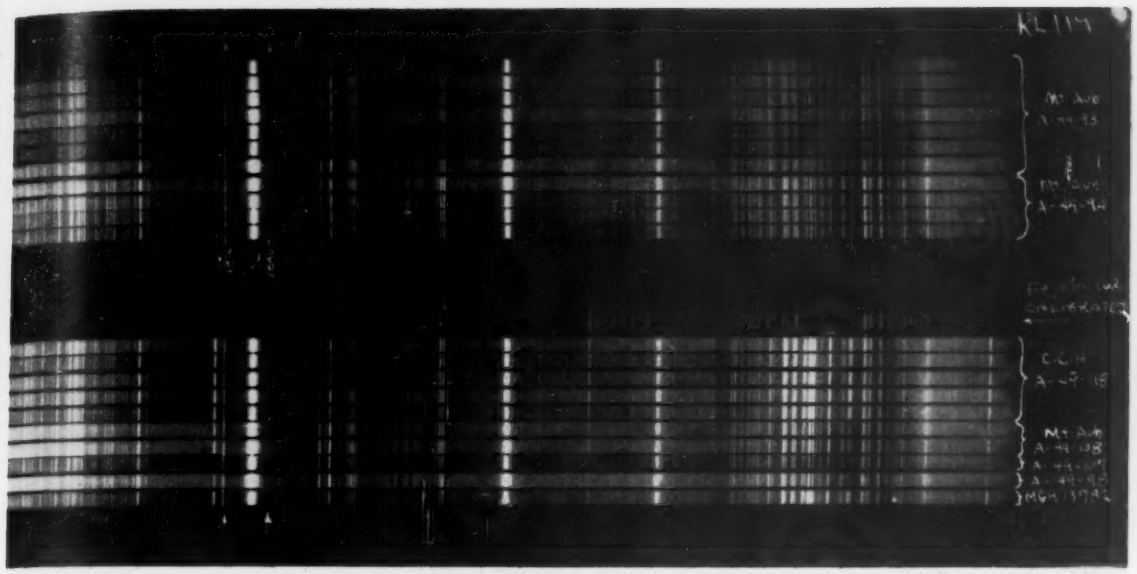
The current-voltage relationships obtained permit both qualitative and quantitative determinations. Several substances may be determined simultaneously under especially favorable circumstances. Instruments now in use make a written or photographic record—known as a polarogram—of these voltage-amperage relationships.

Radioactive isotopes constitute yet another important tool in the study of the physiology of trace metals, but they cannot replace microchemical techniques as some workers have thought. The information obtained by the use of radioactive isotopes is only valuable when simultaneous microchemical determinations are made. When the total content of an element is known, the radioactivity data may provide an index of turnover, and therefore of physiological activity. In this fashion it is possible, for instance, to "label" a given metal enzyme and study its behavior in the body as compared with the total amount of the metal present. It is safe to say that microchemistry and radioactivity studies serve best as mutually dependent, supplementary approaches.

In addition to the techniques described, several others are being explored, the quantitative usefulness of which at the present time is not as yet definitive. *X-ray absorption* in tissues, *ion exchange separations,* and artificially induced radioactivity in tissues by pile bombardment (*"activation analysis"*), all seem promising approaches. The successful and well-equipped analyst of trace metals ought to be able to employ all these methods and not one in preference to the other, because in in-



Littrow (prism) spectrograph. Spectrogram shown in figure following was obtained with this instrument.



Quantitative analyses of human thyroid glands on Littrow spectrograph, using gallium as an internal standard. Notice the stepped iron spectrum for plate calibration. Brackets indicate repeat exposures of aliquots of the same sample; notations identify the samples. This is a positive of an actual spec plate.

dividual cases they all have their very specific and peculiar usefulness. In many instances, they cannot replace one another.

Botanists, microbiologists, and bacteriologists have been more successful than others in delineating the importance of the oligoelements in the species of interest to them. The relatively short life span of these species, and the relative ease of control of their nutrient media, have generally been important criteria. Others have directed their attention to the effect of oligoelement deficiencies on intermediate metabolism by studying isolated enzyme systems of trace-metal–deficient organisms.

To a test organism all essential nutritional components except one are supplied, and the effect of its deprivation is studied. Such a regime naturally requires absolutely pure reagents, and contamination is a very serious problem. Commercial "chemically pure" reagents are never adequate, and spectrography will almost always show them to contain critical quantities of the oligoelements. Many investigators have utilized ingenious devices for their removal, with varying success.

Several microorganisms—e.g., *Aspergillus niger*—are such sensitive indicators of the presence or absence of some elements that their growth response to the addition of an oligoelement to standard media has been made the basis of semiquantitative bioassay methods for the detection of these elements. Little work, however, has been done to ascertain concomitantly the resultant metal distribution in the organism.

In higher animals experiments have usually been restricted to rodents because of the great difficulties associated with the design of metal-free feeds for animals unable to survive on highly purified synthetic diets. Early attempts to achieve clear-cut evidence of the essentiality of an element were foiled by the fact that animals were simultaneously deprived of vitamins. The aim of such experiments on animals, as with microorganisms, has been to eliminate a given element from the diet, thereby decreasing growth significantly, and finally cause the death of the animal at a statistically significant earlier stage of its life cycle. Simultaneously investigators noted the symptoms and pathological lesions that developed, to obtain clues to human disease processes of unknown cause, and to explain the role of the metal in normal metabolism. Elvehjem *et al.* and Day studied the effect of such deficiencies on the activity of various enzyme systems in different tissues to find criteria of deprivation. Suggestive but inconclusive information has resulted from these attempts.

Finally, many studies have been designed to establish blood and tissue levels in animals or man, and to utilize deviations from the norm as indices of intoxication, deprivation, or deranged intermediate metabolism. The study with zinc is a point in case. The following brief survey of zinc metabolism may exemplify the general type and nature of information available in this field, and the methods for physiological approaches chosen.

The first observations of plant disease due to

zinc deficiency were made in 1900, though earlier work of Raulin on fungi had already established the element as a growth factor. By 1933 pecan rosette (variously called "little leaf," "mottle-leaf," or "yellows") could be attributed to a zinc deficiency. This disease of pecan trees, characterized by failure of normal development, chlorotic and mottled leaves, and the development of rosette buds below the dead region of the plant, was a source of major economic losses to Texas and Arizona farmers.

Observations on animals showed that mice required zinc for normal development. A deficiency of zinc in the diet produced impaired growth, alopecia, anorexia, ataxia, inanition, and death. Further experiments on rats revealed a disturbed protein, carbohydrate, and nucleoprotein metabolism. Histological studies on zinc-deficient animals demonstrated hyperkeratinization, thickening of the epidermis, and edema of the skin and mucous membranes of the esophagus and mouth. Attempts to design appropriate diets showed that for rats the daily minimum requirement of zinc is about 40 μg/day.

The use of dithizone and polarographic techniques has established approximate zinc levels in some animal tissues. Most organs, including the pancreas, contain about 20–30 μg Zn/g wet weight; liver, skeletal muscle, and bone contain about 60 μg Zn/g wet weight, and the endocrine organs and lymph nodes contain from 60 to 180 μg/g. Blood values vary from 6.5 to 13 μg/g of erythrocytes, depending on the species. In 100 ml of whole blood one finds about 880 μg of zinc. Seventy-five per cent of this blood zinc is found in red blood cells, 22 per cent in plasma, and 3 per cent in white blood cells, which imparts to the latter an unusually high concentration of the element, since there is only one white blood cell for each thousand red blood cells in circulation (Vallee and Gibson).

Studies using Zn^{65} showed that the greatest turnover of zinc took place in the liver, pancreas, kidney, and pituitary, whereas red blood cells and bone were the only tissue components that accumulate the element. Similar studies of pregnant dogs with the isotope Zn^{65} demonstrated the passage of zinc across the placenta.

The distribution of zinc in various pathological conditions has been studied, and extensive deviations from normal values have been observed in leukemia, methylcholanthrene-induced cancer, and pernicious anemia. The zinc content of leukemic leucocytes is markedly reduced, whereas the content of pernicious anemia erythrocytes is markedly elevated. The redistribution of zinc and other metals in induced cancer suggests a general disturbance of the metal content of neoplastic cells.

In 1940 Keilin and Mann established the only known physiological activity of zinc, namely, that it is a structural and functional component of the enzyme carbonic anhydrase. Their method of metal enzyme isolation and purification established the basis for similar investigations on other metal-enzyme systems. The enzyme, previously known to be present in red blood cells where it catalyzes the reaction $CO_2 + H_2O \rightleftharpoons H_2CO_3$, was first demonstrated to be a metal complex by its reversible inhibition by KCN +, H_2S, NaN_3. That this metal was zinc was further demonstrated by the constant proportions of the element (0.33 per cent) in a purified enzyme preparation gained from ox erythrocytes, and by the fact that a linear relationship exists between the amount of zinc present and the enzyme activity. Protein fractionation of white blood cells performed by Hock and Vallee has recently demonstrated that they contain a hitherto unknown zinc protein, which has been obtained in relatively pure form. Its function is not yet known.

The search for an explanation of the physiological role of the oligoelements has emphasized their association with enzyme systems in living cells. Historically the general thesis of a relationship between metals and enzymes was set up by Otto Warburg twenty-five years ago. The approaches and logic he used, as exemplified in his monograph, have markedly influenced subsequent work in this field. He approached the problem primarily by the use of *inhibitors* of metals, mainly CN^- and CO, upon the activity of enzyme systems. More specific inhibitors have since been added. Direct analysis for metal constituents of purified enzymes was usually avoided; most reliance was placed on the measurement of enzyme activity. Keilin and his school in Great Britain, as well as Theorell in Sweden, however, have attempted to characterize the metal content of such systems.

One group of enzymes contains a metal as an integral part of the molecule (Table 2). There is a fixed amount of metal per molecule of protein, with which it is inseparably associated. The enzyme activity is contingent upon the presence of the metal, which will not dialyze. When the element is split from the protein residue by some vigorous manipulation, all measurable biological activity is lost and is not readily restored by readdition of this or any other metal. Some of the metals are part of porphyrin structures as found in hemoglobin.

This group of compounds has been isolated by

TABLE 2

METAL ENZYMES

Iron
Cytochromes
Cytochrome oxidase* (Warburg's respiratory enzyme?)
Cytochrome peroxidase
Peroxidase
Verdoperoxidase
Catalase

Copper
Tyrosinase (monophenol oxidase, polyphenol oxidase, catecholase, dopa oxidase)
Laccase
Ascorbic acid oxidase
Cytochrome oxidase*
(Hemocuprein)†
(Coeruloplasmin)†
(Hepatocuprein)†
(Milk factor)†

Zinc
Carbonic anhydrase
(Leucocyte zinc protein)†

Manganese
Transphosphorylase

Magnesium
Chlorophyll
Carboxypeptidase*

* Metal content doubtful.
† No demonstrated enzymatic activity.

fractionation with varying alcohol concentrations, changes in pH, ionic strength, temperature, metal complex formation, electric fields, or centrifugation. Such fractionations may be followed by measuring enzyme activity and metal concentration per gram dry weight of protein, until constant ratios of

$$\frac{\text{Metal}}{\text{Protein}} \quad \frac{\text{Enzyme Activity}}{\text{Protein}} \quad \text{and} \quad \frac{\text{Enzyme Activity}}{\text{Metal}}$$

are achieved. In most instances enzymes are measured on the basis of their biological activity, and little if any information can be obtained on their *concentration*. The simultaneous assessment of biological activity and concentration of an integral metal component which may be measured chemically, as exemplified by work on carbonic anhydrase and zinc, should facilitate considerably the quantitative study of enzyme distribution and function.

Proof of existence of an enzyme containing a metal as an integral component may be found in the following criteria:

1) Rising metal/protein ratio on physicochemical purification, until a constant metal/protein ratio is assured.
2) Metal and protein are found in molar proportions.
3) No metal is added from the outside.
4) The ratio of enzyme activity/metal is a constant.
5) Removal of the metal results in inactivation.
6) A crystalline protein with constant metal/protein ratio may be obtained.

Isotope tagging allows the study of turnover rates of such compounds and their kinetics, as exemplified by work of Theorell on catalase and Fe^{59}, and Dawson on ascorbic acid oxidase and Cu^{64}.

On the other hand, a very much larger group of enzymes is in very loose association with a metal, and the criterion of association is the *activation* of catalysis. In this group the specificity of the association is lacking. The metals are dialyzable and may substitute for one another in many instances. The metal ion is not an integral part of the molecule, and in most instances the enzyme is apparently active without it altogether. These facts increase the difficulties of assessing the *biological* significance of these in vitro findings. Some have considered the difference between these two groups of metal-enzymes systems to be quantitative only, representing different modes of bond structure but having similar biochemical meaning. Others have thought of the metal as being involved in the formation of the intermediate enzyme-substrate complex. Whatever the hypotheses, *operationally* the first group lends itself more readily to a definitive assessment of the physiological role of metal-enzymes systems at this time, since experimentally the element and the enzyme may be studied jointly and separately in vitro and in vivo.

Even though there are great difficulties in physiological interpretation of work with the "loose enzyme metal complexes," they are of the greatest theoretical importance in the understanding of catalytic phenomena. Many important observations that provide working hypotheses have been made. Metal ions have long been known to have catalytic activity all by themselves. This activity can be enhanced by the presence of inorganic or organic molecules and proteins, as was shown by Warburg. Models for enzyme catalysis have been evolved in this fashion, and recently Smith has attempted to explain the mode of action of certain proteolytic enzymes on the basis of their interaction with metal ions.

Arginase exemplifies an enzyme that is activated by many different metal ions. Manganese, vanadium, iron, cadmium, nickel, and cobalt have all been found to increase the catalytic action of arginase. However, it is not known which ones of these elements—if any—are the physiologically active ones, though manganese has been assigned this role without any conclusive evidence for this assumption. Succinic dehydrogenase is activated by aluminum, chromium, and cobalt, and certain rare earths, such as lanthanum, samarium, and neodymium. The physiological occurrence of the rare earths is even more obscure than that of most

other elements, and it is therefore virtually impossible at this time to assign any functional meaning to this observation. On the other hand, it emphasizes the argument that many in vitro observations may be of passing interest only.

Though most of the theoretical implications concerning this second group of metal enzymes will have to await clarification, the practical aspects of their behavior are of great significance to the enzymologist faced with specific problems in the interpretation of intermediary metabolism. Oligoelements may accelerate or inhibit reactions selectively. With reference to the latter, one can point out that about 50 per cent of all known enzymes require free sulfhydryl groups for activity: it is well known that metal ions can react with such sulfhydryl groups, thereby inhibiting these enzymes. It is completely unknown whether the body avails itself of the inhibiting, activating, or both actions of metals—or neither—to control enzyme reactions.

The problem of trace elements in living systems cannot be assigned to any one particular field, as this sketchy survey shows. It extends from agronomy through botany, microbiology, animal husbandry, veterinary medicine, animal and human nutrition, through the study of human disease processes. It concerns the chemist and enzymologist, as well as the biophysicist. The problem itself is the best demonstration that all phenomena of life, in essence, constitute a closed system. The source of energy for living matter has been the preoccupation of biochemists in the past. They concentrated their attention on the organic molecules which store and supply this energy. The constituents of these molecules, however, are manufactured from the earth's atmosphere and the components of the soil. These molecules supply the framework and act as carriers for energy, the only factor in our life cycle supplied from an extraterrestrial source. Oddly enough, much attention has been devoted to these complex molecules, but their interaction with simpler inorganic compounds derived directly from the soil has been unduly disregarded. The influence of most of the elements upon assimilation of solar energy in photosynthesis and its utilization in metabolic processes has received less than its due share of scientific attention and is yet to be understood. The skeptics might do well to remember the hormones and vitamins and their influence on modern biochemical thought as sufficient proof that what is quantitatively minute need not necessarily be insignificant.

SMO ON THE AIR

Station	Sponsor	Time
	Sunday	
KLBM, La Grande, Ore.	Baker-Union Department of Health (Research Report)	10:15 A.M.
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.
	(Irregular)	
KBKR, Baker, Ore.	Baker-Union Department of Health (Research Report)	....

The Editor of THE SCIENTIFIC MONTHLY will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

Is the Pacific Sardine Disappearing?

J. L. McHUGH* and ELBERT H. AHLSTROM†

The senior author, a native of British Columbia, completed his doctoral research in 1950 at the Scripps Institution, where he has most recently been associated with the investigation of the sardine, a part of the Marine Life Research program. Previously he was a scientific assistant of the Fisheries Research Board of Canada until he was called into military service. In January 1951 he became director of the Virginia Fisheries Laboratory. Dr. Ahlstrom (Ph.D., Ohio State, 1934) has been with the Fish and Wildlife Service since 1939, and is now in charge of the recruitment research on the California sardine. He is concerned also with the identification of the fish larvae occurring off the west coast of North America and has been successful in tracing a number of complicated life histories.

REACHING a maximum of almost 800,000 tons in the season of 1936–37 and remaining above the half-million-ton level during the ensuing eight seasons, the annual catch of sardines along the Pacific coast of North America declined abruptly in the years following 1945. So sharp was this drop that by 1947–48 the total landings only slightly exceeded 100,000 tons. Alarmed by the disaster that appeared to be threatening this major California industry, which processes about 90 per cent of the total Pacific coast sardine catch, the fishery operators began to expand the research being conducted.

Much valuable research on the sardine had been done and was in progress at the time of the decline, but the work was hampered by a lack of adequate funds and facilities. Both scientists and operators felt the need of expanding the research at sea, especially in the oceanographic field.

During the spring of 1946 the San Francisco Sardine Association allotted $30,000 for an investigation of availability of sardines off San Francisco. The purse seine vessel *Pearl Harbor,* equipped with echo-ranging gear as an aid in locating sardine schools, made a number of cruises off central California. Scientists of the California Division of Fish and Game and the U. S. Fish and Wildlife Service who participated in these cruises made observations on temperature and plankton distribution. Only limited information was secured on sardine availability, but the industry was impressed with the value of oceanographic fisheries research, notwithstanding its high cost.

To provide additional research funds the California sardine operators in 1947 voluntarily proposed a levy of 50 cents per ton on sardine landings, in addition to a similar tax then in effect. A Marine Research Committee, consisting of nine members representing the industry, scientists, and the people of California, was set up to administer the new funds. This body was largely responsible for securing from the California legislature a grant of $300,000 (later about $400,000) annually to the University of California for marine life research, with particular emphasis on the analysis of oceanographic conditions that may determine the productivity of the ocean waters.

The augmented sardine research program is a unique cooperative venture in oceanography and fisheries biology. Major contributions are being made by the Bureau of Marine Fisheries of the California Division of Fish and Game, the South Pacific Fishery Investigations of the U. S. Fish and Wildlife Service, and the Scripps Institution of Oceanography of the University of California. In addition, the California Academy of Sciences is studying sardine behavior.

The work at sea, which forms so important a part of the expanded program, requires a fleet of vessels capable of cruising the open Pacific and carrying out their duties under almost all weather conditions. For this purpose, four ships are en-

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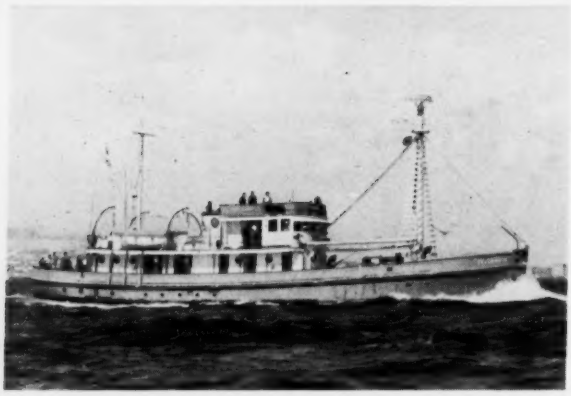

FIG. 1. The *Yellowfin,* research vessel of the Bureau of Marine Fisheries, California Division of Fish and Game, engaged mostly on studies of the Pacific sardine.

gaged exclusively in the collection of data. These are the *Yellowfin* (Fig. 1), a 114-foot, wooden-hulled ex-Army freight and supply ship, of the California Division of Fish and Game; the *Black Douglas* (Fig. 2), a 133-foot, steel-hulled ex-schooner, of the U. S. Fish and Wildlife Service; and two ex-Navy vessels, of the Scripps Institution (Fig. 3), the *Crest,* a wooden-hulled 136-foot minesweeper, and the *Horizon,* a steel-hulled, 143-foot tug.

Periodic cruises with three of these vessels are made throughout the year over an area extending from Oregon to central Lower California and to a distance of approximately 400 miles offshore. Each ship is at sea for a period of about two weeks in every month, occupying a series of 40–50 oceanographic stations (Fig. 4). At these stations, from observations of temperatures and salinities at 15 depths from the surface to 1,000 meters (Fig. 5), the course and velocity of the oceanic currents are estimated. Samples of water collected from each depth are analyzed for dissolved oxygen and phosphate content. Hauls with fine-meshed nets (Fig. 6) are made to determine the kinds, abundance, and distribution of small organisms, including fish eggs and larvae. Material is also collected for determination of the amount of microscopic plant life.

Previous Investigations

The Bureau of Marine Fisheries of the California Division of Fish and Game has been investigating the resource for nearly a third of a century. Within this period this agency has followed its growth from its beginning as a major fishery during the first world war, through its period of rather uniform expansion until 1936–37, through the years of seeming stability, to the recent epoch of reduced abundance. As a result, this is one of the better-documented fisheries in the world.

The changes in the sardine population brought about by a rapidly increasing fishing pressure have been observed since 1918. It was apparent following the 1934–35 season that the catch per unit-of-effort was decreasing, even though the total catch continued to rise and maintained a high level for ten years (Fig. 7). Despite improvements in vessels and gear, which greatly increased the efficiency of exploitation, greater effort was being expended to catch equivalent amounts of fish.

The California Division of Fish and Game has made many other important contributions. Prior to 1938 its scientists investigated sardine spawning over much of its range off California and even off Lower California, and established that the area off southern California was an important spawning center. In cooperation with agencies in British Columbia and in the states of Washington and Oregon, they conducted a tagging program, which showed that individuals move from southern and central California to British Columbia and from the latter region to California. It was concluded from these and other studies that the fisheries from British Columbia to southern California, and even as far south as central Lower California, were exploiting the same stocks of fish.

In 1938 the U. S. Fish and Wildlife Service set up the South Pacific Fishery Investigations, with headquarters at Stanford University, for the purpose of augmenting research on this rapidly growing fishery. This group focused its attention on the problem of age determination in sardines and on the success of recruitment in relation to oceanographic conditions. Criteria were developed by which the annual marks on scales and otoliths

FIG. 2. The *Black Douglas,* research vessel of the South Pacific Fishery Investigations, U. S. Fish and Wildlife Service, employed in recruitment and oceanographic research of the Cooperative Sardine Research Program.

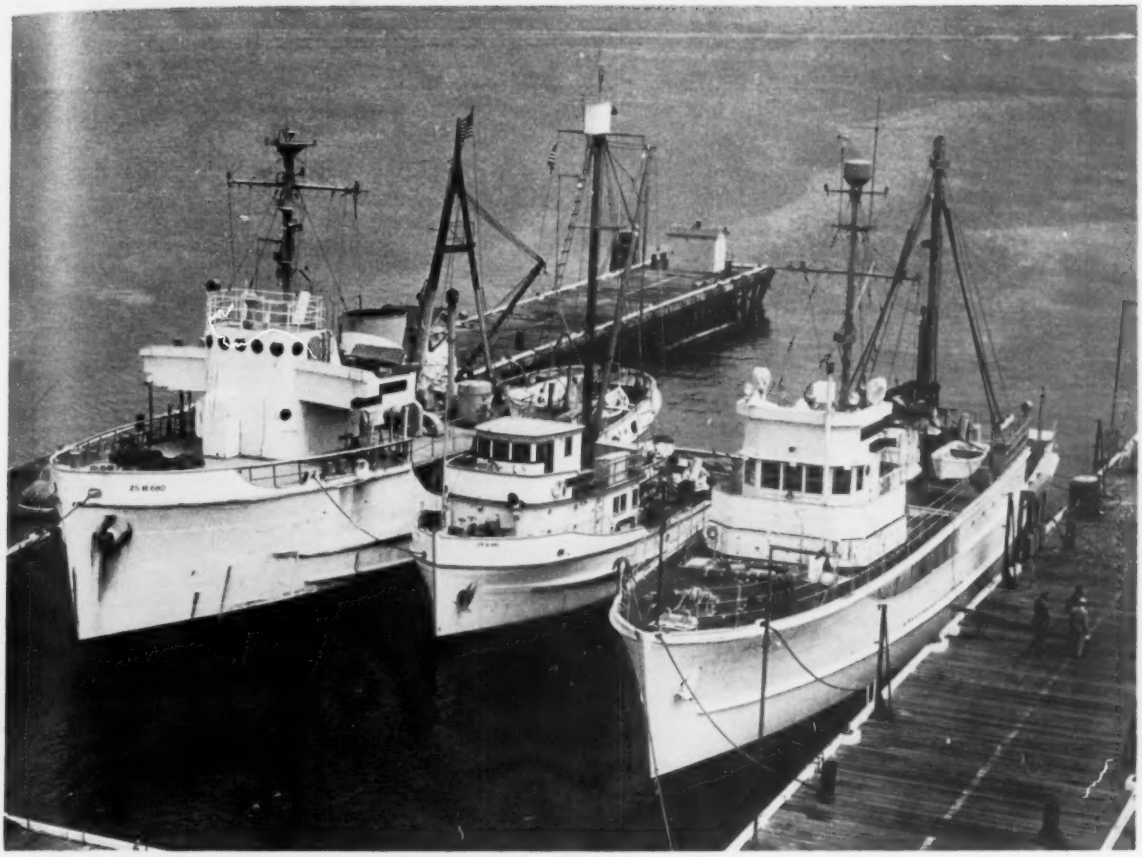

FIG. 3. The *Horizon*, left, and *Crest*, right, of the Scripps Institution of Oceanography, University of California, take part in the oceanographic and recruitment studies of the Cooperative Sardine Research Program. The *Paolina T*, center, has also been used in certain phases of the program.

can be recognized, and thus the age composition of the catch determined rather accurately. The study of the age composition of the commercial catch soon developed into a cooperative project between the U. S. Fish and Wildlife Service and the California Division of Fish and Game and is now being carried on routinely, with the assistance of British Columbia and the states of Washington and Oregon. Undertaken as a cooperative program with the Scripps Institution of Oceanography, the recruitment study foreshadowed on a modest scale the research that forms so important a part of the current expanded program. Such cooperation characterizes much of the Pacific sardine research.

Cause and Effect

Most of the fish in the commercial sardine catch are from one to five years old. During the period since 1941, covered by routine age analysis, only about 4 per cent of the fish have been six years of age or older. The age composition of the catch in California waters changes with the season, the fall fishery taking mainly young fish and the average age increasing as the fishing season progresses. The average age of fish taken in the several fishing ports also increases from San Pedro to British Columbia. In the Pacific Northwest, fish have been available in quantity only during the summer months, whereas off California sardines are taken commercially from August through February. These facts have been interpreted as indicating that the fish move northward each summer and return in the fall and winter, the older fish reaching higher latitudes. The seasonal changes have not been as marked in recent years as formerly, mainly because the proportion of older fish in the population has been considerably reduced (Fig. 8). Before the recent decrease in landings, about 60 per cent of the catch in numbers was made up of fish three or more years of age, but since 1946 these ages have contributed only about 33 per cent of the total. In California waters fish of one or two years of age have become the chief support of the fishery, whereas in the Northwest the change in age composition in conjunction with a decline in abundance has led to the collapse of the fishery.

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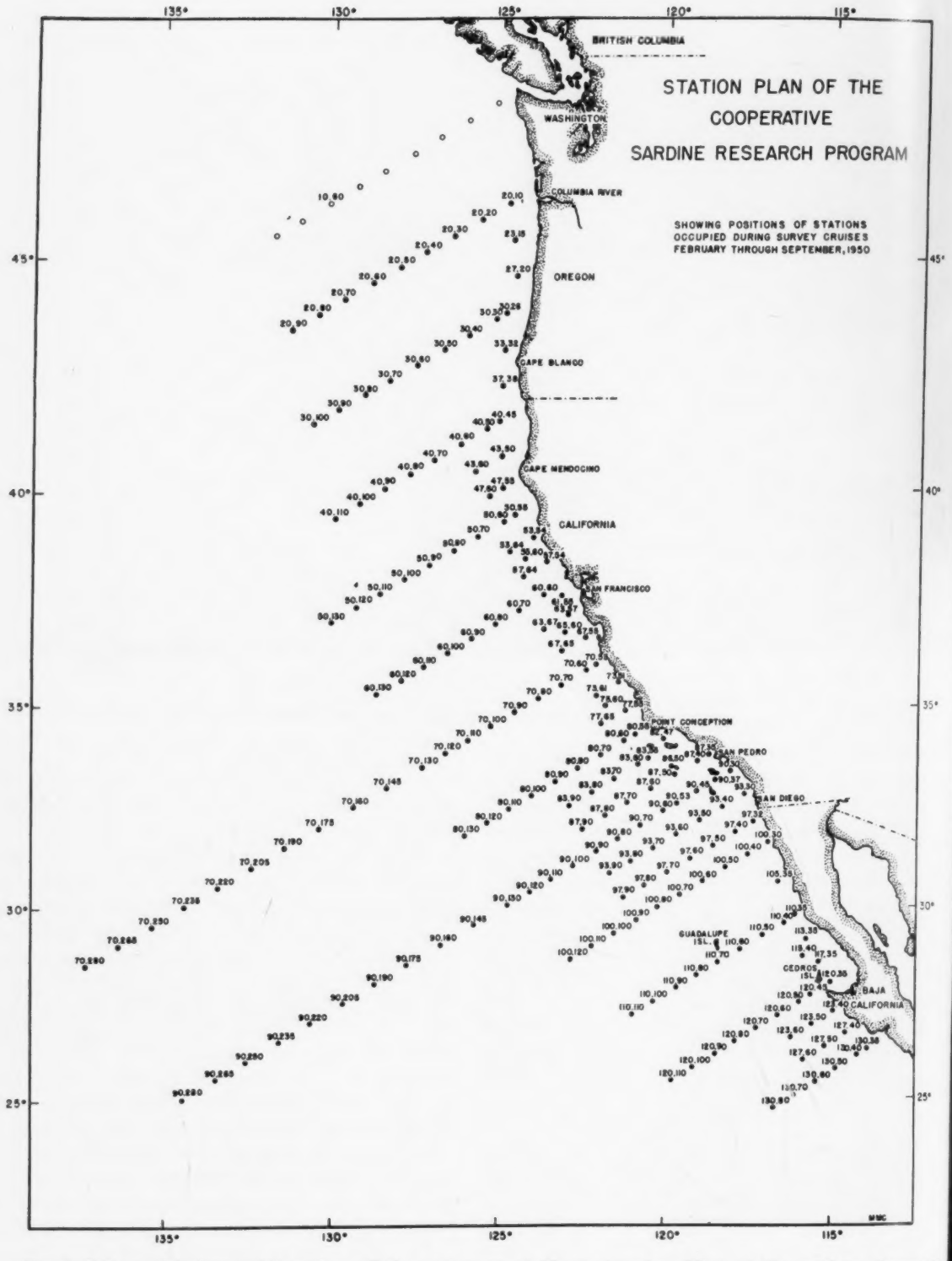

FIG. 4. Pattern of hydrographic stations off the west coast of North America. Most of the stations shown are occupied at monthly intervals by the vessels of the Cooperative Sardine Research Program.

FIG. 5. Nansen bottle attached to the wire preparatory to making a hydrographic cast. A series of 15 such bottles is used at each station to measure temperature and to collect water samples for determining salinity, oxygen, and phosphate content at appropriate depths from the surface to 1,200 meters.

As pointed out previously, the Pacific coast sardine fishery has passed through three phases: a twenty-year period of rapid growth, a decade of fairly constant yield at a high level, and a sudden and alarming decline amounting almost to collapse in all fishing ports except San Pedro. That the peak season of 1936–37 was followed by a succession of fruitful years of fishing was due largely to a succession of good spawning seasons, climaxed by the influx of an unusually large year class spawned in 1939. Had it not been for these successful year classes, the collapse might have come much sooner. On the other hand, continued spawning success might have considerably delayed the collapse or prevented it entirely. In addition, the decline in landings was accentuated by poor survival of the adult fish, or by a change in availability.

It is characteristic of pelagic-spawning species such as the sardine that the survival of young from a season's spawning varies greatly from year to year. Those that survive in a very favorable year contribute many more times the number of fish to the commercial catch than do the recruits from a poor year. During the early years of the fishery these variations in recruitment, although recognized by the biologists, were hardly obvious to the fishermen, because the fluctuations were hidden by the presence of relatively large stocks of older fish. Concurrently with increased fishing pressure—and in the opinion of many because of this pressure—this buffer was effectively reduced.

If the causes underlying the variations in survival of recruits from year to year can be detected, the success of each year class may be determined a year or two in advance of its entry into the fishery. The value of such information to the industry is recognized. The hope that such predictions could be made was a dominant factor in prompting the industry and the legislature to provide funds for expanding the research.

Objective of the Expanded Program

The main objective of the expanded sardine research program is to understand the sardine in relation to its physical and chemical environment, its food supply, its predators, and its competitors, and to evaluate such findings in terms of the survival of young and in terms of the distribution and availability of the sardines when they reach commercial size.

FIG. 6. Bringing the plankton net on board at completion of a haul. These nets, of fine-meshed silk, are provided with a meter in the mouth to measure the amount of water strained. They capture eggs and larvae of the sardine and of many other fish, in addition to providing a sample of the small organisms on which the sardine feeds.

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Environmental relationships. Fluctuations in the sardine supply cannot be understood clearly without considering the interrelationships between the species and the physical, chemical, and biological conditions under which it lives. It is the task of the physical oceanographers in the program to provide data that will lead to a better understanding of the oceanic circulation off the Pacific coast of North America. Of particular importance is the necessity of measuring and recording seasonal and annual fluctuations in the horizontal and vertical circulation patterns. Both types of circulation may have significant effects on the distribution, survival, and availability of young and adult fish. The factors that control the vertical circulation are of particular interest in that they provide the mechanism by which the nutrient-rich subsurface waters, characterized by low temperature and high salinity, upwell into and fertilize the upper layers where plant growth takes place. Marine plants, the basic food producers of the sea, are confined to a surface layer seldom deeper than 300 feet, the depth of effective light penetration. The plants utilize certain essential chemical raw materials, such as nitrates and phosphates, that must be replenished constantly, or productivity will be low. It is now known that the duration and intensity of upwelling vary considerably from year to year and from place to place. An understanding of these variations may provide a clue to the distribution and success of year classes of sardines.

Survival and recruitment. Much of the research being conducted cooperatively by the Scripps Institution and the Fish and Wildlife Service is directed toward determining the causes underlying the success or failure of year broods of sardines. It is well established that survival varies considerably from year to year, for there appears to be no very consistent relation between the size of the spawning population and the number of recruits that it provides for the commercial fishery. The factors that effectively influence the survival of a year's brood of young are diverse and complicated. On hatching, the young sardine carries with it from the egg a supply of yolk that lasts only a few days. Before this food supply is exhausted, the tiny threadlike larvae, not much more than a sixth of an inch long, require, lest they perish, a supply of suitable food near at hand. This food must be small and must be present in quantity in the right place and at the right time, for the larvae have small mouths and are not active swimmers (Fig. 9). During the larval period they are subject to predation and competition from many sources, and perhaps also to epidemic disease. It it presumed that

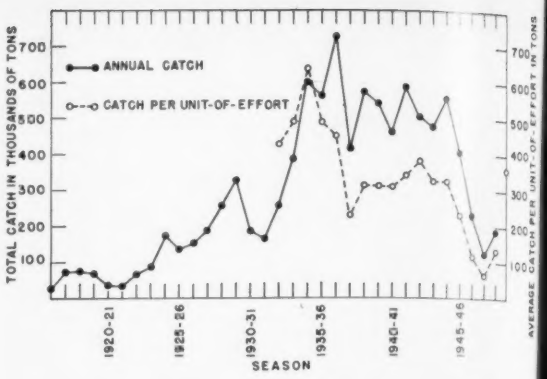

FIG. 7. Annual sardine landings in thousands of tons for the entire Pacific coast, illustrating the early expansion and recent decline of the fishery. The average catch per unit-of-effort is shown from 1933 to date.

during these early stages the greatest mortalities occur.

There is some evidence that the area of spawning may vary considerably from year to year and that these annual variations have a bearing on the success of spawning. Although a large proportion of the eggs are deposited in two major regions off the coasts of southern California and central Lower California, in certain years highly successful spawnings may take place elsewhere. In the early summer of 1939, for example, unusual numbers of nearly ripe sardines were taken in the fishery off Washington, and in the spring and summer of 1940 tremendous concentrations of young sardines of the large 1939 year class were observed in the waters of British Columbia and Washington. It is possible that young of this year brood were more abundant in the waters of the Pacific Northwest than in the usual nursery grounds off southern and Lower California. The success of the 1939 year class may, therefore, have resulted from an unusually wide distribution of spawning. Under such conditions the chances for survival of young fish should be greatly increased, for a much greater bulk of food would be available over the larger spawning area, and the chances would be greater that some groups of larvae would encounter particularly favorable conditions.

In order that the population be maintained at a constant level of abundance, only two recruits resulting from the spawning of each female need survive to spawning age. A female sardine spawns about 100,000 eggs per season, and her total production may amount to several hundred thousand eggs. Assuming that the number of eggs produced by a female during her life span is 200,000 and that two fish resulting from her spawning reached commercial size, the resulting mortality from egg to

adult would be 99.999 per cent. If survival were increased 100 per cent, thus doubling the population, the total mortality would be reduced by only 0.001 per cent. In this connection it is possible that the oceanographic studies will provide information on variations in currents and in the physical and chemical properties of the ocean waters that may be correlated with the varying production of sardines.

The problem of availability. Adult sardines are more available to the fishery during some years than in others. This source of fluctuation in the commercial catch is being investigated by the California Division of Fish and Game. The problem is basically one of sardine distribution in relation to physical, chemical, and biological oceanographic conditions. Schools located by echo-ranging and echo-sounding are sampled to determine the size and age-composition of the species present. In addition, oceanographic observations are made and plankton hauls taken for comparison with the stomach contents of the fish. These observations fit into the larger framework of the cooperative research at sea.

Predicting the strength of year classes. The strength of a year class probably can be measured at any stage following larval development, although not with equal facility during the younger stages. In the past a reliable measure has been possible only after the year class has entered the commercial fishery. Since this method has limited predictive value, it is important that estimates be made in advance of recruitment into the commercial stocks. The problem is being given considerable attention in the research program. Two methods of estimating the abundance of young sardines are being investigated. The first, employing quantitative plankton-collecting procedures, will provide a measure of abundance and mortality during the early stages when the young fish are randomly distributed. The second, based on surveys on the nursery grounds, will provide an estimate of relative abundance of schooling young four to six months old. Several other lines of investigation may provide data for forecasting the strength of year classes. Physical and chemical observations may be found to fit patterns that vary in the same way as year-class strength. For example, there may be a direct correlation between intensity of upwelling and year-class strength, for which presumptive evidence has been found in the annual variation of salinities off the coast.

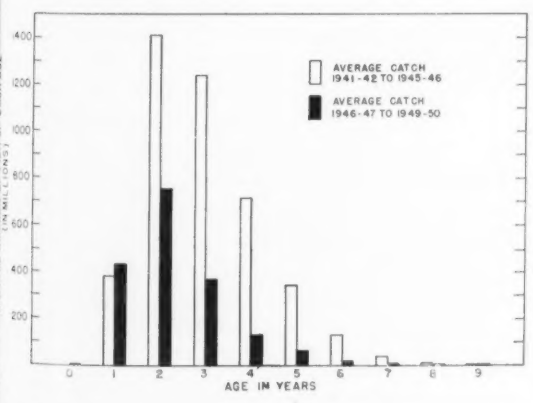

FIG. 8. Age composition of the sardine catch before and after the recent decline in sardine abundance.

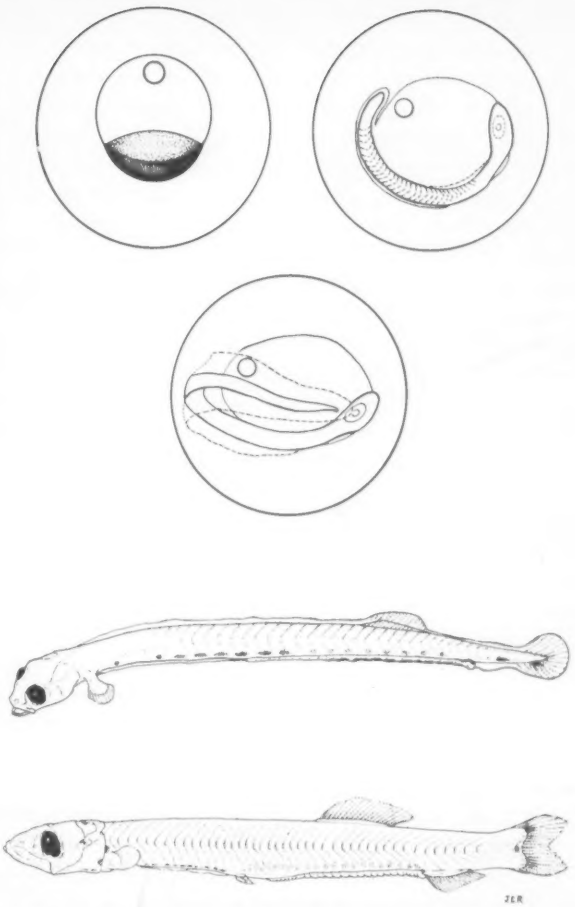

FIG. 9. Stages in the development of the egg and the larva of the Pacific sardine. The egg is approximately 1.7 millimeters in diameter. The two larval stages are about 10 and 25 millimeters long, respectively.

Recent Developments

Some important results have already been obtained. It has been confirmed that sardine spawning takes place in water of rather specific temperatures, almost entirely within the narrow limits 13°–16° C. The importance of the southern Cali-

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fornia region as a spawning area has been re-emphasized, and further information has been gained on a second region of possibly even greater importance, lying to the south of Cedros Island off Lower California.

The surveys are yielding valuable information on the distribution, abundance, and life history of many species of fish. Much of this information should be of value to the sardine industry in indicating latent marine resources capable of being exploited in the lean years. Since there is a limit to the total amount of protein food that the ocean can produce, decrease in abundance of one species may be accompanied by proportional increases in others. If this be so, we cannot take full advantage of our fishery resource as long as we exploit only a part of it by concentrating on certain species. Consequently, the fuller utilization of such under-exploited fishes as anchovy, saury, and jack mackerel should be encouraged. Major expansion of these fisheries should be accompanied by adequate biological studies. The information being gathered in the present surveys would be of considerable value in planning the research program and would contribute to the solution of the problems involved.

A development that will have far-reaching implications in oceanographic fisheries research has been made in the estimation of ocean currents. A method has recently been perfected by means of which the effect of rhythmic variations in density, apparently associated with tide-producing forces, may be eliminated from the determinations. Charts of current patterns, formerly characterized by complicated series of eddies, thereby have been considerably simplified.

The requirements of the cooperative program have stimulated the development of many new and improved types of oceanographic equipment. Outstanding is the high-speed plankton sampler, a device that can be operated at top cruising speed, and that gives a continuous record of both depth and the amount of water strained. To carry this instrument down and to maintain the desired depth an especially designed bronze depressor is employed. These and other developments have excited considerable interest among scientists in many fields.

The Pacific sardine is not disappearing. Fish populations, whether exploited commercially or not, are subject to rather wide fluctuations in abundance. The decline in sardine abundance seems to have been due to a succession of poor spawning seasons coincident with a change in the availability or an increase in the natural mortality of adults. The European pilchard and the Japanese sardine fisheries have suffered similar declines in abundance. The catch of Pacific sardines during the current fishing season may reach 350,000 tons, indicating that a substantial recovery is taking place.

ILLUSTRATIONS: Fig. 1, Leo Pinkas; Fig. 2, Orthello L. Wallis; Figs. 3, 5, and 6, John MacFall; Figs. 4, 7, and 8, drawn by Margaret Calderwood; Fig. 9, drawn by Janet Roemhild.

LIZARD

Reality? A lizard on a fence
Pants in the sun; the split pine-rail drips gum;
Over his beady eye the sky's immense.
Reality is here, throbbing and dumb.

Upon which actual scene our words intrude,
Speaking of hormones or the meddling State:
A tragic or a comic interlude.
The lizard in the great sun waits, will wait,

Ambassador of life base as our blood—
Baser, since all his heat's the sun's direct.
Reality? We've never understood,
Although we talk, cry havoc, and suspect.

HAROLD GRIER MCGURDY

Modern Statistical Science and its Function in Educational and Psychological Research

PALMER O. JOHNSON

Palmer O. Johnson, whose teaching fields are statistics and research methods for graduate students, is professor of educational psychology at the University of Minnesota. His background training was in mathematics and in the physical and biological sciences. He took his Ph.D. in 1928 at Minnesota and in 1934–35 was a volunteer worker with R. A. Fisher at the Galton Laboratory. His research fields are statistical methods and experimental design, with particular reference to education and psychology. His article is based on the vice-presidential address delivered before Sections I and Q at the 1950 AAAS meeting.

The nature of modern statistical science. There was a time when statistics as a tool in experimentation was almost completely ignored by the experimenter; in fact, it was regarded as "introducing unnecessary confusion into otherwise plain issues." This position, although still persisting in some quarters, gave way to that where the statistician was called upon to examine the findings of experiments after they had been carried out. He was asked to determine from the data presented to him certain average values and their "probable errors" considered as connected to the averages obtained. This was essentially the conception of the role of a statistician at the time R. A. Fisher was appointed statistician at the Rothamsted Experimental Station in 1919. It is significant that the need for a statistician was first recognized about seventy-five years after the experimental station had been founded (in 1843) by Lawes and Gilbert. It is also highly significant that someone of the scientific genius of Fisher was appointed to this position. For Fisher was not willing to do only what he was told to do. He recognized that in order to analyze observational data it was necessary to know the conditions under which they were collected or, rather, that the statistician devise the plan for their collection. This led to the formation of certain principles of experimental design, including the golden rule that statistics must be brought into play in the advance planning stage of investigations. In the latest development in sequential analysis, statistical considerations enter into the actual process of experimentation itself. The method, applied first for military and industrial purposes in World War II, is applicable when replications can be processed one at a time. Briefly, the procedure is to use the accumulative information from each replicate to end the experiment just as soon as the findings are decisive.

The three principles of experimentation—replication, randomization, and control of variability—have stood the test of time and are having increasing applications in the experimental sciences. Fisher developed simultaneously the technique of the analysis of variance, which supplies the appropriate method of estimating the experimental error and carrying out exact tests of significance. In this connection it is fitting to point out the contribution of "Student" to the refinement of the classical theory of errors. Student showed how, by determining the exact sampling distribution of a statistic, allowance could be made for its sampling errors. He thus inaugurated what are known as exact tests of significance. Furthermore, since these tests are based on the appropriate degrees of freedom, he removed the restriction that for rigorous conclusions the sample must be "large." It is usually held that 1908, the year of Student's classical publication on the probable error of "the mean," is the beginning of modern statistical science.[1]

The principles and the technique of analysis developed in experimentation have also played a

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dominant role in the development of modern sampling theory, leading to efficient sampling designs of censuses and surveys.

The developments cited have been made possible by the great advances in the theory of statistical inference. Statistical methodology has become a particularized, but significant and well-defined, branch of scientific method. Among significant branches of the subject of statistical inference are statistical estimation, tests of statistical hypotheses, and experimental and survey designs.

In this article we shall emphasize directly and by implication the use and potentiality of modern statistical science in the design and analysis of educational and psychological investigations. We are interested not only in the strictly applied function, such as in educational experimentation that aims at recommendations for educational practice, but also in the formulation of educational or psychological laws or principles that transcend the merely descriptive aspects of observable processes. These principles serve to summarize the workings of the educational processes and in so doing may give new insight into the processes themselves.

With such a comprehensive subject and within our justifiably limited space, we can discuss only a small and probably biased sample of its content. The population is stratified into three areas, which we shall call design of experiments, sampling theory and practice, and techniques for analysis of data.

The Design of Experiments

Statistics as scientific method. Scientific method is common to all the sciences. Statistics as a branch of the scientific method provides the doctrine of the planning of experiments and collection of other types of observations, and of interpreting their consequences. This common relation to all sciences is strengthened in that statistics furnishes a common methodological basis of understanding, namely, the derivation of the best infallible inference which the experiment or other observational data possess with respect to the theory or hypothesis under examination. This means that statistics is concerned with the central problem of any scientific investigation. Further, the extent of agreement between experimental data and the assumptions is among the objective factors that govern the acceptance of solutions proposed for scientific problems. In this determination statistical procedures play a prominent part.

The broad problem of statistical inference is the determination of the uncertainty of conclusions drawn from experimental or observational data. The theory of probability is basic in the attack upon this problem, by providing probability statements concerning the correctness of the conclusions drawn. The theory of statistics includes other content besides the theory of probability, such as the principles of estimation, the theoretical effects of the randomization principle, and the principles of testing statistical hypotheses.

In contrast to the theory of statistics there is the practice of statistics. The number of tools and techniques being placed at the disposal of the research worker is continually increasing. As scientific research develops, experiments become more complicated and the statistical tools more complex and specialized. The mathematical statistician faced with a particular experimental problem sets up some sort of model representing those features of reality considered most essential. In his choice of procedures he has recourse to the principles of the theory of statistics. He has the responsibility of making clear to the practitioner the assumptions underlying the valid use of the model and the statistical tools required for testing their fulfillment with the view of their appropriate application in the situation at hand.

The theory of design of experiments aims at the establishment of principles for collecting the primary data such that valid inferences may be drawn from them and for extracting the maximum amount of information contained in the data in the most efficient manner. Before considering this problem we shall present certain transitions in concepts concerning experimentation as a means of adding to our scientific knowledge.

Transitions in scientific models. Nineteenth-century scientists worked on the principle that, proceeding step by step, it is possible to isolate systems and processes. The physicist regarded his laboratory as an isolated system, at least for ordinary accuracy. By abstraction the laboratory became the place for the isolation of causal processes by controlling the state of the environment. The uncontrollable nuisance variables, with all their uncertainties, which did not lend themselves to control and quantification, were eliminated by the device of abstraction. The assumption that abstraction and isolation of the problem are necessary conditions of the scientific method of experiment can no longer be regarded as axiomatic. The experimental method as developed in physics and chemistry proved to be effective in meeting the needs of the day, although the number of variables to be controlled is found to be continuously increasing as the complexity of matter becomes more and more apparent. Abstraction is difficult enough in the physical sciences, but it is almost totally ineffective in

studies of the behavior of living organisms. The living organism is primarily adaptive, and responses alter subsequent responsiveness; hence the problem is confounded by the organism and its environment. If the organism is taken out of its setting into the laboratory, the problem is ruptured. With these venerable antecedents, the laboratory came to be accepted as the phase of science that stands for control. Its primary purpose was to arrange circumstances such that data could be collected undisturbed by unknown factors of any substantial size. With this dominance of the laboratory in the techniques of control, the conclusion was drawn that the laboratory was a necessary condition for controlled experimentation. This belief in the laboratory led the physicist to set up his methods of investigation as the model for scientific investigation. As we shall point out later, the biologist turned to this model in the attempt to apply the experimental method to the solution of his problems.

With the developments in methods of control during the past twenty-five years, the older conception of the laboratory as the *sine qua non* of the experimental method has been severely challenged. This transition is based on an unwillingness to accept as obvious the idea that laboratory techniques are either *sufficient* for securing answers to critical questions or *necessary* for securing reliable data on a specified scientific question.[2] In recent years the standard laboratory has been particularly attacked by technology because laboratory tests are carried out under such arbitrary and specialized conditions that the information provided by them cannot be used for making valid predictions of field conditions. The proponents of the laboratory reply that the conditions of service are not all known and not known precisely enough to make accurate predictions. Consequently, service conditions are just as highly specialized as those in the laboratory.

It remained for the development of experimental designs based on appropriate statistical theory, and advancing research in methodology, to show how the controversy between the laboratory test and the "conditions-of-actual-service" test could be resolved. Modern statistical science repudiated the assumption that in order to apply the experimental method it was necessary to keep all conditions the same with the exception of the experimental factor under study. It showed how through the application of the experimental principles, previously indicated, an experimental problem can be investigated when several variables are undergoing change. Through the principles of design and the correspondingly appropriate technique of the analysis of variance and covariance, it becomes possible to vary as many factors as may be regarded as important and still draw exact inferences applicable to "actual conditions."

The model for experimentation in physics set forth by Galileo in his famous study of the fundamental laws of falling bodies served as the only one for more than three hundred years. It served physics well. Newtonian mechanical science advanced enormously for more than two hundred years with hardly any changing concepts and evolving conceptual schemes. The Galilean model was passively accepted by the biologist, who followed the formula: Establish controlled conditions such that all factors except one can be held constant and then study the effects of this single factor. Mills' canons were presumed to afford the criteria by which it could be determined whether there was a causal relation among specific phenomena.

The history of science indicates that at times a scientific model once accepted may remain unchallenged for long intervals of time—long after it has fulfilled its original purpose. It may be recalled, for example, that Bayes' theorem, which was the first attempt to rationalize the process of inductive reasoning, set down in 1763, was first seriously criticized by Boole in 1854. Scientists are averse to abandoning a plan that may frequently lead to plausible conclusions, and above everything, for which they have no practical replacement. It remained for Fisher in the biological sciences to demonstrate that what was needed was no mere restatement of current procedures in experimentation in more rigorous terms, but a fundamentally new approach to experimentation. It was not until 1926 that modern principles of experimentation were laid down.

Comprehension of the role of presuppositions is basic to an understanding of science. What can be answered and what must be presupposed in order to give answers are in a very real sense basic problems in scientific inquiry. Likewise in the case of the newer statistical techniques there are no general criteria specifying *what* variables are to be changed, nor operational criteria assuring no effect on the results of the noncontrolled or noncontrollable factors. These criteria are supplied by the methodological demand for a complete specification of conditions whereby the operations can provide adequate answers to the scientific problem. The research worker must thus translate his problem into questions precise enough so that the specific observations may be evaluated relative to their adequacy, and also general enough in his presupppositions about the aspects of nature not

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actively controlled to enable him to decide if a continued sequence of observations would approach some limiting value. The experiment is said to be "controlled" in a formal sense if the preceding formal conditions are satisfied. It may be stated that only a beginning has been made in the specification of formal presuppositions, but that the existence of such controls for the process of discovery to become scientific holds equally true whether the experiment occurs inside or outside a laboratory.[2]

The advances in the design and analysis of scientific experiment are of special significance in the social sciences, where it may be difficult or impossible to set up a "social laboratory." In the fields of psychology and education the possibilities of evaluating the complex problems encountered in their more natural setting offer new promise to research in these fields. The point is, not that the laboratory experiments should be abandoned, but that they in turn will take on new significance in identifying factors sufficiently promising to be combined in more comprehensive experiments designed to permit ecological as well as populational generalizations.

The limits of the generalizability of experimental evidence provide an excellent basis for comparing the traditional, or "classical," experiment and the modern multivariate experiment. The classical type of experiment traditionally employs one value, with some tendency recently to employ only a few carefully selected values of the stimulus variable. At times the subjects of the experiment may be chosen as representative of a well-defined population. Here there may, therefore, be populational generality, with minimum statements of the form, restricted to "specific conditions." The desideratum of the modern experimental design is that of obtaining in addition to a sample of subjects chosen to be representative of the population a sample of stimulus situations that may be regarded as ecologically representative.[3]

Modern designs of experiments are particularly appropriate in large-scale cooperative experiments in education. By coordinating the researches of several different investigators in representative samples of schools, it becomes possible to obtain not only information on the individual investigations but also the additional knowledge resulting from the combination. The application of modern designs leads to detection of the genuineness of discrepant results obtained at different places, or at different times, and thus to recognition of the limitations of generalizations that might otherwise be accepted uncritically.

We turn now to a discussion of certain designs of experiments, with illustrations of their applications.

Experimental principles. The term "experiment" generally refers to two kinds of experimental problems: (1) a search for what are presumed to be absolute constants—e.g., the determination of the velocity of light—and (2) the comparative experiment, which consists of the application and comparison of differential treatments or various technical procedures. It is the latter type we shall consider here.

The purpose of the experiment may be used as a distinguishable characteristic. An experiment may be needed to make an immediate administrative decision, which must be made no matter how indefinite the result of the experiment may be. For such a purpose, the individual responsible for making the decision designs the best experiment he can and makes the decision he thinks best. He is not, or at least supposes that he is not, interested in whether the design of his experiment may lead to decisions that are correct in the long run. On the other hand, an experiment may have as its purpose the advancement of scientific knowledge, no immediate action being under consideration other than the possible projection of future experiments. It is experiments that have this latter purpose that we have in mind here. The process of adding to scientific knowledge by experiment consists in the repetition of the following sequence of events:

1. The critical examination of theories on the basis of available evidence.
2. The formulation of hypotheses that are testable or appropriate for testing by experimentation.
3. The carrying out or execution of experiments.

The experiment can eventuate in giving answers to questions of the "yes" or "no" type by means of tests of significance, and of the type calling for values or estimates of specified unknown populational parameters. The planning of experiments involves the following:

1. The statement of the purpose of the investigation, which may be given in the form of the questions whose answers are sought, of the hypotheses under test, or of the effects to be estimated. A specification of the population for which the conclusions apply should be indicated.

2. The description of the experiment, including such salient features as the selection and specification of the treatments, the accuracy of the measurements, the selection of the experimental units, the designation of the general conditions underlying the experimental test, and the specific experimental design to be used.

3. A description of the method proposed for drawing conclusions from the experimental results. This could well include an outline of the analysis of variance, the indication of the tabular form for presenting the results, an

account of the tests of significance proposed, and the treatment differences to be estimated. This provides a verification of the treatment comparisons regarded as relevant to the specified purposes of the experiment. The possible outcome of the experiment should be anticipated, and the corresponding conclusions formulated prior to the carrying out of the experiment. This is essential if any inference drawn from the experimental observations is to be supported by a fiducial probability statement. Apparently significant but unanticipated inferences can only be used for suggesting later experiments.[4]

An experiment must be designed such that its interpretation is based on its own evidence. A primary requisite of such an experiment, a self-contained experiment, is the necessity of providing its own control; that is, the need of basing all conclusions with reference to the differential effect of two or more contrasting treatments on the differences in the response or reaction of two or more similar bodies of experimental material. Experiments become comparative and not merely absolute, through the effective use of controls. The rationale underlying the use of controls is that they are used to exclude, at a designated probability level, a number of alternative admissible interpretations of the experimental results—possibilities that individually and collectively have an unknown probability of having occurred. The second requirement of a self-contained experiment is that it must provide a valid estimate of the experimental errors that really influence the comparisons made. Only when the experiment makes possible this estimate can tests of significance be applied to the experimental results—tests that are disconnected from all past experience and therefore capable of adding new knowledge.

The first requirement of an experiment planned so that a valid test of significance can be used in the interpretation of experimental results is replication, which is needed not only to estimate, but also to reduce, error.

There must be an element of randomization in an experiment, since an experiment is performed to test a hypothesis, or to estimate a parameter or a set of parameters. The hypothesis adopted is supported by odds based on a calculation assuming random sampling. So, also, the estimation of the parameter by a confidence interval is based on the assumption of randomness. Randomization, along with replication, ensures that the error estimates are valid and that the estimates of treatment means are unbiased. Randomization makes it possible for the results of the experiment to be presented as objectively as possible, so that the number of assumptions and judgments required in interpretation is reduced to a minimum. This principle of Fisher's is unique to modern experimentation.

In summary, controls, replications, and randomization are the essential principles basic to a self-contained experiment.[5, 6]

The role of the analysis of variance. The introduction of the analysis-of-variance technique by Fisher provides a convenient arithmetical procedure for pooling estimates of errors from different treatments and simultaneously for eliminating the variation due to blocks or to other features of the experimental arrangement.

In the mathematical model underlying the formal analysis of many experimental results, each observation may be represented as the sum of four components attributable, respectively, to the general mean, the effect of the treatment, certain environmental effects which the design of the experiment makes it possible to isolate, and the residual effect, or experimental error. In the same manner the analysis of variance partitions the sum of squares of the observations into four sums of squares corresponding to the four components listed above. Briefly, the analysis of variance furnishes a short-cut way of obtaining the error sum of squares. The sum of squares for treatments is required for the F-test of the null hypothesis that there are no differences between the effects of the treatments. The analysis may be extended to provide the sums of squares needed in testing differential effects of a subgroup of treatments. Finally, the component attributable to environmental effects may be used to estimate the increase in accuracy of the experiment brought about by the removal of these effects from the treatment means. The division of the total number of degrees of freedom, the independent comparisons, corresponding to each of the component sum of squares calculated, show the logical structure of the experiment.

We may now go from the principles of experimental design and the appropriate statistical analysis to certain specific experimental designs, with some examples.

Specific experimental designs. In planning experiments one proceeds in accordance with the principles to fit a specific design to a projected experiment. There are no general rules leading to the selection of designs. Each experimental situation presents its own problems. A practical working principle is to use the simplest design best meeting the needs of the experiment. This does not exclude the use of complex designs when these best serve the experimental purpose. Mathematicians, at least since the time of Euler, have shown interest in combinatorial problems dealing with the arrangement of a finite number of things in sets, or patterns,

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satisfying specified conditions. It remained, however, for R. A. Fisher to show how to put these configurations to work.

The primary aim of the experimental design is to minimize experimental errors. Numerous procedures have been developed over the past twenty-five years. In their recent book *Experimental Designs,* Cochran and Cox describe 150 experimental designs.[4] Designs are classifiable into a few standard types. The newer developments in the field have mainly been specialized devices for specialized purposes, which do not depart in any fundamental way from the general principles of design as we have discussed them. We can describe only briefly here the main types of design.

The basic idea underlying planned grouping of experimental material is rather simple. The subdivision of the experimental material into relatively homogeneous series is fundamental, as is the principle that the process of subdivision can be advantageously duplicated. For example, take an experiment with a number of separate replications. With respect to any replicate, it should be noted that the experimental errors of the results from any replicate originate only from sources of variation affecting the experimental units within the replicate. Accordingly, the error of the difference between two treatment means determined over a number of replicates must also originate entirely from the variations within the individual replications. The fluctuations from one replicate to another do not go into the error component. In conducting an experiment, this elementary fact is used in attempting to control sources of variation affecting different units in the same replicate. The reduction of differences among the replicates is not required. This specific design is known as randomized blocks. In this design each treatment occurs equally frequently, more commonly once in each block, and the treatments are randomly allotted to the experimental units within the block. The term "block" refers to any group containing the required number of experimental units.

A simple example of this type of design, which at the same time brings out another striking advantage of modern designs is one phase of a more comprehensive study by Burt and Lewis.[7] This study was concerned with the relative merits of different methods of teaching backward readers. Four different methods of teaching the backward readers who had IQs between 79 and 83 and reading quotients between 61 and 73 were evaluated—alphabetic, kinesthetic, phonic, and visual. There were 48 experimental subjects, chosen at random from schools in which one of the teaching methods had been predominant. The data were classified according to (1) the teaching method adopted in the remedial work (the "treatment"), and (2) the teaching method to which the child had previously been subjected in his own school ("school"). This cross-classification gives 16 subclasses in all. There were three individual determinations in each subclass; caution in the design made it possible to evaluate the interaction between remedial treatment and previous teaching method used. Briefly (Table 1), a significant difference was found in the mean effects of the different remedial methods used. The interaction was significant, which means in this case that the results achieved with any given remedial device depended on the method by which the child had been originally taught. On the whole, a change in teaching procedure was more productive than renewed efforts with an old teaching method by which the child had previously been taught. This provision in some types of experimental designs, making it possible to measure interaction among factors, is a unique contribution.

The basic idea underlying planned grouping is carried forward to an additional stage in the Latin square design. In the design, the treatments are arranged diagrammatically so that each treatment occurs once in each row and once in each column of a rectangular array. The arrangement of the treatments is chosen at random from some class of possible arrangements. The variations among the groups of experimental units corresponding to the rows as well as those corresponding to the columns are eliminated from the experimental error. This design is particularly advantageous when the distributing effects of two factors need to be eliminated from the experimental comparisons.

TABLE 1

ANALYSIS OF VARIANCE TABLE FOR EXPERIMENTAL RESULTS OF BURT AND LEWIS' SECOND EXPERIMENT ON REMEDIAL READING

SOURCE OF VARIATION	*D.F.*	SUMS OF SQUARES	MEAN SQUARE	*F*
Remedial treatment	3	880.1175	293.372	9.32*
Previous methods (schools)	3	230.6225	76.874	2.44
Interaction	9	764.9167	84.991	2.70†
Residual or error	32	1007.5733	31.487	
Total	47	2883.2300		

* Significant at 1% level.
† Significant at 5% level.

This type of design may be illustrated by the simple experiment of Thompson,[8] evaluating four

different methods of testing spelling ability—the multiple choice, the second dictation, the wrongly spelled word, the skeleton word. The pupils were divided into four groups equal in spelling ability as determined by an initial dictation spelling test. There were also four lists of words of presumably equal spelling difficulty. The arrangement was such that no group of pupils had the same spelling list as another group, and each list was used exactly once. The scores were the number of words spelled wrongly in the initial spelling test but correctly in the "treatment test" (Table 2). A significant difference was found to exist among the spelling tests. Any disturbing effects due to differences in spelling ability among groups of pupils and in spelling tests were eliminated from the experimental comparisons.

TABLE 2

ANALYSIS OF VARIANCE OF THE EXPERIMENTAL RESULTS ON THE STUDY OF DIFFERENT SPELLING TESTS AS REPORTED BY THOMPSON

SOURCE OF VARIATION	*D.F.*	SUMS OF SQUARES	MEAN SQUARE	*F*
Lists of words (rows)	3	359.5	119.8	1.19
Groups of pupils (columns)	3	74.5	24.8	4.07
Tests (treatments)	3	4626.5	1542.6	15.26*
Error	6	606.5	101.1	
Total	15	5667.0		

* Significant at 1% level.

The Greco-Latin square is an arrangement whereby the treatments are grouped into replicates in three different ways, with the result that the effects of three different sources of variation are equalized for all the treatments. Grant gives examples of this design as applied in psychological experiments.[9] An example shows how three different distributing factors—variation in list difficulty (e.g., nonsense syllables), order or practice effect, and consistent differences among subjects in memorizing ability—are eliminated from experimental error.

The skeleton of the experimental design is shown by the division of the total number of degrees of freedom (127) as follows:

SOURCE OF VARIATION	*D.F.*
1) Experimental condition (Latin letters)	3
2) Variation in list-difficulty (Greek letters)	3
3) Order of practice effect (columns)	3
4) Individual differences between S_s (rows)	31
5) Error	87
6) Total	127

The two methods—randomized blocks and Latin squares—form the chief foundation for the experiment. Other methods are elaborations on this principal foundation.

One of the most important designs is the factorial. In this design all combinations of a set of factors are included in the experiment. The comprehensive experiment involving all treatment comparisons may be laid out in randomized blocks or in a Latin square. The effects of the factors may or may not be independent. In case they are independent, all the information is contained in the main effects. In dealing with factors that are independent, one procedure (the traditional alone) is to set up separate experiments each dealing with a single factor. An alternate procedure is to include all factors simultaneously in a factorial experiment. In contrasting the two methods, it may be said that factorial experiments will result in considerable saving of time and materials. This is because (1) when factors are independent the main effects alone give the total effect of variation in the factor, and (2) the main effect of each factor is estimated with the same precision as if the whole experiment had been devoted to a single factor alone.

When factors are not independent, the simple effects of any factor will fluctuate, dependent upon which particular combination of the other factors is used. In such cases a series of separate experiments devoted to each of the factors could give no information at all on the interaction effect of factors. With the factorial experiment the effects of every factor are evaluated for every combination of other factors included in the experiment. Thus a great amount of information is obtainable both about the effects of the factors and of their interactions. Thus, in experiments where the purpose is to make recommendations applicable over a wide range of conditions, the factorial design may be especially advantageous, since it can lead to wider ecological generalizability of experimental evidence.

Solandt *et al.* describe a well-conducted experiment which illustrates the advantages of factorial design.[10] This experiment was undertaken to determine the effect of variations in electrical stimulation on the reduction in atrophy of denervated muscle. The experiment tested the efficacy of four types of electric current, four lengths of treatment periods per day, and three numbers of treatment periods per day in all combinations. Accordingly, there were $4 \times 4 \times 3$, or 48 different treatment combinations, in a single replication. The 25-cycle current produced consistently the best results, and the 60-cycle current consistently the second-best. The

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variation in the number of treatments given daily brought about significant variations in response, and the effectiveness increased with the number of treatments applied daily. No sensible differences in outcomes were found by varying the length of treatment. None of the interactions was found to be significant—e.g., the effect of a change in type of current was not dependent on the number of treatment periods or on the time interval of a treatment period.

Johnson and Tsao carried out an investigation of educational development by the use of a factorial design.[11] The experiment was planned so that supplementary data were collected and used to reduce the experimental errors by eliminating two sources of variation, not controlled by the design, through the technique of covariance analysis. The specific design was a $2 \times 3 \times 3 \times 3$ factorial type. The factors selected for study were the two sexes, three scholastic standings, three orders of individual educational development, and the three senior high-school grades. Mental age scores and initial test scores were the independent variables. The type of experimental design is that in which absolute replication is dispensed with and hidden replication is involved.[6] The highest order interaction is employed as the basis of testing the significance of the lower order interactions. There were fourteen hypotheses tested in the experiment, four concerning main effects and ten concerning interaction effects. The criterion score used as a measure of the stage of educational development was based on a composite score made up of the scores on nine separate tests. The analysis led to the conclusion that only scholastic achievement and individual development were significantly related to total educational development when adjustments were made in the final measures of educational development for variations in mental ability and initial educational status. The difference between the adjusted mean development scores of the sexes was significant at the 5 per cent level.

Many of the new developments in experimental design are connected with factorial design. The total number of treatment combinations increases rapidly with the introduction of additional factors, or of additional levels of a factor. With this increase it becomes increasingly difficult to secure homogeneous replications for a randomized blocks design. In this case, also, the Latin square requires usually an impracticable number of replications. As a consequence, the size of the experimental error per experimental unit increases.

The device of confounding and the use of split plots for subsidiary treatments were early developments aimed at avoiding the increase in experimental error. The fundamental principle common to the new developments is the construction of a "block" smaller than a complete replication. The designs make possible the elimination of error from the differences among the smaller blocks. The reduction in size of the block is gained, however, only at the expense of accuracy obtainable in certain treatment comparisons. The discussion of the details of these designs cannot be taken up here. The interested reader is referred to the excellent treatment by Cochran and Cox.[4]

Sampling Theory and Practice

The sweeping theoretical and technical advance leading to modern experimental designs produced a startling advance as well in the development of sampling designs and techniques.

Experiment and survey contrasted. Before taking up sampling developments we wish to distinguish between the roles of experiment and survey in research. Both involve patterns by which observations are collected, but in experimental designs the aim is to estimate the effects of applied treatments, whereas the objective in survey work is to estimate certain characteristics or parameters for a particular, defined population. A clear differentiation should also be made between the kinds of conclusions that can be drawn with certainty and those that are speculative. In surveys, when a relationship has been established and estimates made of it, no assertion of causality can be made. Thus in a nutrition survey, if it is found that the children of large families are less well fed than children of small families, the definite conclusion can be drawn that size of family is associated with malnutrition, and estimates can be made of the degree of malnutrition that exists among children of families of different sizes. It cannot be concluded, however, that size of family is the cause of the malnutrition. This is the case, although the fact that the income per person is automatically less, if a fixed total exists, would induce one to suspect an underlying causal relationship.[12]

Under certain conditions the disturbance attributable to clearly defined extraneous factors that may influence the estimates of the effects of other factors may be removed by statistical methods of analysis. However, there remain other undetermined, or perhaps unascertainable, factors that cannot be taken into account. Experiments are essential for determining causality. Surveys may be of great value, however, in circumstances that do not yield to experimentation. They may also serve through preliminary work to identify factors worth experimenting with.

Sampling problems and principles. Quite the most fundamental problem in practical statistics is that of sampling. The planning of sampling designs is usually involved in two situations: experimental investigations and survey studies, descriptive or analytical. In both situations the problem is that of securing accurate and representative samples of the population. Since the fundamental purpose of sampling is to obtain information about a population, the first matter of importance in either experimental or survey investigations is to begin with a clear definition of what that population is. It follows that it is equally important to have a clear understanding of what is to be found out about the population. A specification of the population and the decision as to the precise purpose of the investigation usually determine certain parameters of the population to be estimated or certain hypotheses to be tested.

The problem of inductive inference is defined by the object of the experiment or the sampling survey: to secure the answers to certain clearly defined questions. Since it is impossible or impracticable to investigate the entire population, the investigator examines a small part of it. On the basis of the information provided by the sample, inferences are drawn for the whole population. We have previously pointed out the particularly difficult but all-important sampling problem for the experimenter in education and psychology to secure samples populationally and ecologically representative.

Turning our attention now more specifically to the sampling survey, we find that the statistician must concern himself with securing precision, and particularly with conditions affecting accuracy. The two aspects of the problem of estimating an unknown parameter of a specified population are:

1. The sample has already been drawn, and it is required to find a function of the observations that is a best estimate of the parameter(s).
2. The sample is not yet drawn, and the problem is to determine the number of observations to be drawn from the population and what method of sampling should be used. Involved also is the practical problem of selecting the method which will provide the desired degree of accuracy at minimum cost.

Broadly speaking, there are three methods of sampling:

1. Unrestricted random sampling, where each member of the population has an equal chance of being included in the sample. The term "random" implies that all possible samples of a given size have the same probability.
2. Stratified random sampling, where the population is subdivided into a finite number of strata, and from each stratum are drawn a predetermined number of observations by random sampling.
3. Purposive selection, where the individuals are selected in accordance with some control criterion.

We see reflected here at least two principles of experimentation previously discussed:

1. Arrangement of the materials into relatively homogeneous strata, utilizing prior knowledge of the population, previous to drawing the sample.
2. The principle of randomization: Randomness is the property of a sample that makes it possible to calculate the probability of error of the inferences drawn. The theory of probability cannot be applied to a sample that is not randomly taken, so that it is not possible to measure the degree of confidence to be placed in any inference drawn from a nonrandom sample. Deming's discussion of probability samples and judgment samples[13] is informative.

The statistician meets considerable and, at times, unpredictable difficulties in sampling human populations, particularly with respect to the condition of randomness.

The introduction of the principles of randomization and of the analysis of variance as the technique of analysis of sample observations has made possible the attainment of unbiased estimates of the quantities under survey and of determining the errors to which the estimates are subject. The analysis of variance, through making it possible to pool estimates of error and to separate components of error that are not homogeneous, has reduced to a relative few the number of independent sampling units required to be taken from each quantity of sampled material. This has made possible the use of relatively complicated sampling designs frequently involving samples in two or more stages.

The method of purposive sampling was at one time used rather extensively in sample surveys. No rigorous rules of selection were specified, with the outcome that many purposively selected samples were found to be by no means equivalent to balanced random samples. The practice was to select sampling units with values of the character near the mean. The variability of the known quantitative characteristic, as well as of other characters closely correlated with it, will clearly be considerably less than the real variability in the population. This again emphasizes the fact that randomization is necessary for trustworthiness.

Sampling in educational and psychological research. Most workers in educational and psychological research seem to remain completely indifferent to these great fundamental advances in the theory and techniques of sampling. Marks has pointed out that it was difficult to find any study in psychological research that used sampling designs making possible the unbiased estimate of the amount of sampling error.[14] He used the description of the sampling methods in the revision of the

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Stanford-Binet Scale to show how the design could not give statistics with measurable standard errors.[15] Mention may be made here of the minimum sampling unit in educational and psychological studies, which is often the class, grade, or some other grouping. The technical sampling method used is that of cluster sampling. Cluster sampling almost always increases the sampling error as compared with unrestricted random sampling. This is due to sampling previously existing groups of the population, which practice involves a positive intraclass correlation of the variable under investigation. Since cluster sampling is an extremely valuable method in educational and psychological research, it is necessary in using it to know the conditions and means by which statistical estimates and the measures of their sampling error may be accurately ascertained.

There is often an urgent need in education to obtain information quickly on some current problem. Often a sampling study is definitely advantageous over a complete count because of its reduced cost and greater speed. It may also actually give more accurate results than a practicable complete enumeration. Also, the characteristics of the population under study may change over the longer interval of time required for complete count, processing, and publishing the survey results. Sometimes, of course, it may be difficult or impossible to sample the population concerning which information is sought. Thus, in sampling voters' opinions as a basis for predicting election results, the population to be sampled is the population of opinions of voters at the time they go to the polls. Since the sample must be obtained at least several days before the election day, it is possible to sample only the population of opinions of intending voters sometime before election. The opinions, and the intention to vote, are both subject to change without notice.

An example of a sample survey study to obtain information in a short time is the investigation by Cornell.[16] Information was needed as soon as possible after the fall opening concerning the enrollment of students in six major classes of higher educational institutions. Cornell applied the method of stratified random sampling, determining the number of sampling elements to be randomly taken from each stratum on the basis of the product of the size and the standard deviation of the stratum.

The writer has had several Ph.D. theses carried out under his direction that have used modern sampling procedures in conducting analytical surveys of the achievement of students in various high-school subjects. Roughly speaking, the procedure is to develop examinations reflecting the principal objectives of the field and to give them to a representative sampling of classes in the schools, first in September and again in May. An intelligence test is also administered. The cooperation of the State Board of Education has been excellent in carrying out these studies, and almost complete sample returns have resulted. Through some of the principles of sampling and methods of analysis discussed in this paper, it has been possible to identify a number of factors significantly related to achievement. For example, Anderson in the field of science was able to identify a number of significant factors existing in the pupil, in the teacher, and in the teaching practices.The cooperating teachers kept a log of their work and provided the requested information concerning student and teacher.[17]

Techniques for the Analysis of Data

Finally, let us consider certain techniques for the analysis of data that are the product of modern statistical science. Our first consideration will be given to the development of methods of multivariate analysis. It will not be possible to include in this brief résumé a discussion of the methods of factor analysis as statistical procedures often used in attempts to solve problems in psychology.

Multivariate analysis. The methods of multivariate analysis have been found useful in a variety of problems occurring in biological, psychological, and economic research. The ordering of things into classes is basic to empirical science. The general problem of classification is a discrimination problem. Fisher first introduced linear discriminant functions in 1936 in his paper "The Use of Multiple Measurements in Taxonomic Problems."[18]

Suppose that n_1 random individuals of one species have been measured on each of k characteristics and that n_2 random individuals of another species have been similarly measured. What linear combination of these measurements would discriminate best the members of one species from those of the other? What linear function,

$$I = \lambda_1 X_1 + \lambda_2 X_2 + \ldots + \lambda_k X_k,$$

would serve to identify the species to which each single individual belongs? The discriminant function I is based on the mathematical principle that the coefficients λ_i, $i = 1, 2, \ldots k$, be determined such that in an analysis of variance of I the ratio of the sum of squares between species (groups) to the sum of squares within species (groups) is a maximum. Then quite simple mathematical calculations provide the numerical values of the λs required.

The need for such a function is also encountered

in experimentation where several variables are used. For example, in growth experiments different measurements of the experimental subjects may be taken over a series of weeks. Here several concomitant variables are also measured and used in the analysis of the experimental results. In the design of such experiments a first step is to specify which variables are to be considered as dependent and which as independent. Through the application of the technique of the analysis of covariance the independent variables are used to reduce the unaccountable variation in the dependent variables. Then the analysis of variance applied to the adjusted dependent variables is used jointly to derive the linear function of the corrected values which is most highly sensitive to treatment differences. This is achieved by discriminant analysis.[19]

Later work on the discriminant function has extended the scope of the technique. Instead of discriminating between two populations, the problem may be to discriminate among several. A single parameter is sufficient to distinguish between means for two different populations, but two discriminant functions are required for three populations, and in general $(P\text{-}1)$ parameters are needed for P populations.

Rao has developed statistical criteria for classifying an individual as a member of one of many groups to which he can possibly belong. He also treated the problem of arranging groups into a hierarchical system through the application of the generalized distance function (D^2) originally developed by Mahalanobis.[20] In the case of two groups, D^2 is the square of the difference in mean values of the best linear function of the measurements of any number of characters. Rao introduced the concept of a quantitative measure of separation which, when satisfying certain fundamental requirements, can be specified as the distance between two groups. These measures of distances can be used to sort out clusters or constellations of groups, and it becomes possible to state that any two groups of a constellation are in some sense closer to each other than any two belonging to different constellations.[21]

Cochran and Bliss[22] have shown how to use variates which themselves have no discriminating power but may be used to advantage in improving the discriminating powers of other variates. For example, it is suggested that in an investigation of the differences introduced by training, the IQs of the subjects might be taken as concomitant variates in discriminating with the aid of measurements taken at the end of training. Thus, after the measurement of the IQs, a sample of pupils may be divided at random into two groups and each group then subjected to a different type of training. Discriminatory powers will be increased if the two groups are selected so as to have the same, or as nearly as possible the same, mean IQ values, so that the generalized distance function D^2 is small.

The solution of multivariate problems has been greatly facilitated by the development of mechanical and electrical devices for carrying out calculations, without which much of the work would be long and tedious or even impossible. World War II developed machinery that brought about dramatic economies in time and manpower and rendered accessible to numerical analysis certain problems that otherwise could not be attempted. Development of methods for finding the inverse of the matrix, and of latent roots and latent vectors, has also greatly facilitated the use of the large number of variables available in certain fields. Hotelling has reviewed the main types of matrix calculation needed for application. The mechanical and electrical devices for performing needed calculations, the iterative and direct methods, the calculation of errors, and the devices of partitioning large matrices are thoroughly treated in his highly informative paper.[23]

To appreciate the analytical powers of some of these statistical tools developed for multivariate analysis, we shall sketch briefly the analysis made by Rao and Slater.[24] These methods of analysis probably have many psychological and educational applications—for instance, in selection.

The data were obtained by recording the incidence of a number of "pointers" found to occur significantly more frequently among neurotic than among normal officers in the Army and Navy. The problem reduced to the classification of five "neurotic groups" and one "normal group" on the basis of three measurements. The statistical problem is that of allocating an individual specimen to his proper group and of arranging the groups themselves in a hierarchical system.

The first step in problems of this kind is to test whether the observed differences between the groups are significant. Here, since there were multiple scores, the method of analysis of dispersion was used rather than the method of variance—covariance analysis applied when a single criterion score is available and where concomitant variation is to be eliminated. The next major step was to calculate all possible values of the generalized distance function D^2. These values make possible the comparison of each group with every other and the arrangement with respect to each

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group in increasing order of magnitude. The test of significance was then made for each D^2. It was found that the six groups could be arranged in approximately linear order, with "normal" and "obsessional" groups at the extremes and "personality changes," "anxiety state," and "psychopathy" groups coming in between in the order given.

The next step consisted in determining whether the differences between the groups could be attributed to a single linear function. For this determination it is necessary to use the methods developed by Fisher for calculating the values of the direction cosines which define the line giving the closest fit to the mean values of the group. This involves the solution of a determinantal equation, giving the roots of the equation. This information, supplemented by appropriate tests of significance, was used in answering the question whether the mean values of the six groups were collinear. The hypothesis of collinearity was accepted. Had the hypothesis been rejected, one would test for coplanarity and so on until a nonsignificant residual variation is reached.

In the present study, only the first root of the determinantal equation was found significant, but in investigations of this kind it is often important in practice to consider at least some of the smaller roots in the determination of the configurations of the various groups. In this analysis, therefore, the canonical variates corresponding to the first two roots were calculated. The mean values of the first two canonical variates were calculated for each of the six groups, and a pictorial representation of the groups presented, analyzed, and the psychological interpretation given.

The last process in the analysis consisted in establishing the statistical criterion for determining the group to which an individual, upon whom the primary measurements were available, is most likely to belong. The use of a chart for this purpose was described, but the shortcomings of this procedure were indicated and the best method was illustrated. This method uses all the measurements and derives directly from them the appropriate criterion for the allocation of an individual to the group to which he most likely belongs. The general solution of this problem is given elsewhere by Rao.[21] The computational aspects are given with respect to the present problem. The linear discriminant scores were calculated for each of the groups. In practice, then, given the three primary measurements of an individual, his linear discriminant score may be calculated and he is assigned to the group for which his score is highest.

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SCIENCE ON THE MARCH

UNUSUAL PETRIFICATION IN TROPICAL PANAMA

THE earth has been literally swarming with land animals since Paleozoic time, yet bones other than those of very recent origin are rarely found upon its surface. The combined agents of decay, chemical weathering, and mechanical erosion are constantly acting upon all objects that constitute the earth's surface and are in the process of converting them to new and more stable chemical compounds. All organic and inorganic materials, since time began, have been modified in some degree by these natural processes; even mountain ranges have been leveled by them.

Of the untold billions of land animals that have lived and died since the Paleozoic, only an infinitesimal fraction died under circumstances where they were temporarily insulated from these disintegrating processes. The tar pits, ancient spring holes, swamps, and shallow inland seas have all trapped a few archaic land animals from time to time and preserved them for man's study, but it is probable that for every species classified from these sources hundreds of others have remained unknown.

Bones may contain as much as 80 per cent insoluble mineral matter, of which calcium carbonate seldom exceeds 10 per cent; most of the remainder is calcium phosphate. The remaining 20 per cent includes the organic cell structure that holds the bone together. In a tropical region, with abundant moisture and constant high temperatures, the organic content of a bone will decay rapidly. When bones are exposed to these conditions, the decomposition of the organic matter is easier to predict than the behavior of the calcium carbonate. In the average soil of the tropical jungle there is an abundant supply of humus and coarser organic matter that liberates large quantities of carbonic acid and consequently dissolves the lime content of bones at a fast rate. This solvent action probably lags behind the decay of the organic tissue under all circumstances, but in the tropics it proceeds rapidly enough so that a bone on the jungle floor or in the surface soil disintegrates in a very few years. Old Indian tombs in Panama usually have no bones, but the archaeologist can determine former location of the bodies by the arrangement of the pottery, the stone tools, and the position of the ornaments.

In spite of the accelerated rate of decay, in this tropical region we find perfectly preserved bones upon the earth's surface and in shallow graves that are hundreds of years old.

The surface bones are found in ancient shell mounds where perfectly preserved specimens of deer, fish, turtle, bird, and small-animal bones have been uncovered. The fragile bird bones, fish vertebrae, and other small bones were so well preserved in every detail that we found it difficult to believe that they were old, yet they were considered to be about five hundred years of age. We knew that shell mounds in the Temperate Zone contained bones, but when we considered the constant high temperatures and abundant moisture of our tropical environment we could not overcome the feeling that nature was playing a trick on us.

The bones in graves came to our attention when a bulldozer, working on an Army post, accidentally uncovered an Indian burial site. The ancient graves had been dug through a layer of black humus which contained many sea shells and then down into coarse, shell-bearing sand. The contact of the sea shells with the humus had produced a high concentration of calcium bicarbonate in the soil water that passed down through the graves. The dissolved lime began going out of solution as calcite cement even before it left the lower edge of the humus layer, and continued to deposit more intensely as the solutions passed through the coarse sand. Because of this, patches of sand were found adhering to the bones, pottery, and other artifacts. Chemical tests showed that this durable stucco finish was produced by calcite cement. Perfect skeletons of the ancient Indians were found in which the calcium carbonate content varied from 10 to 20 per cent. The porous bones, as one might suspect, had acquired the greatest amount of calcium carbonate. Seashell necklaces and ornaments fashioned of bone and drilled animal teeth were perfectly preserved by the depositing lime, and the exact position of nose ornaments, necklaces, chest plates, and arm bands on the interred chieftain was never in doubt. Plates of food offerings still held the fragile bones of birds and small animals. The location of the pottery and stone tools in reference to the body could be seen at a glance, and the ques-

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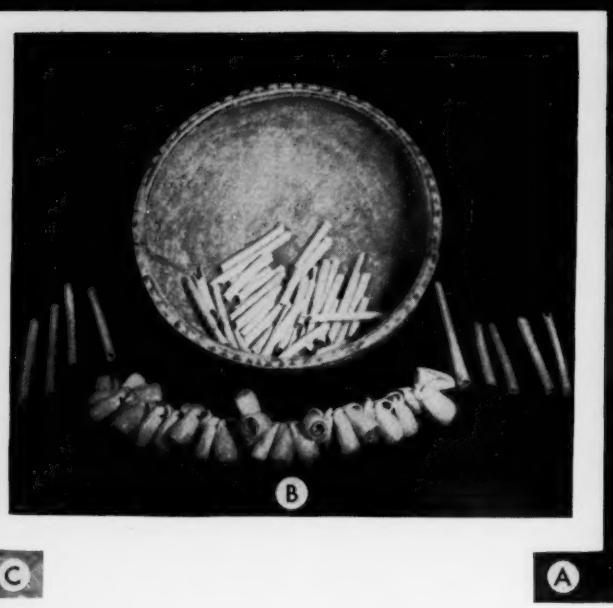

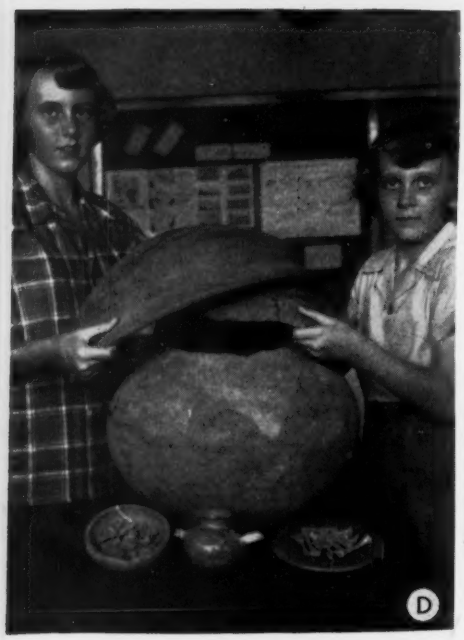

A: When 3-*N* HCl comes in contact with the surface of an excavated urn, the vigorous effervescence is quick proof that pottery is thoroughly impregnated with calcium carbonate. Pottery that did not come in contact with the lime-bearing solutions crumbled so completely that it could not be reconstructed.

B: Decorated pottery and cut sections of fragile bird bones, along with cut and drilled seashell ornaments, are all preserved for archaeological study by the action of lime-bearing solutions.

C: Penetrating the pottery, lime-bearing solutions fill the pores with calcite, a cementing process that resulted in its preservation during centuries of soaking. Patches of sand on sides of large urn were cemented there by excess deposits of $CaCO_3$.

D: Large urns such as this were filled with food and placed in the grave with the departed Indian chief. The restored lids, held by author's daughters, were badly broken as the soil settled. The spouted pot and plates of food rested safely inside the urn for more than 500 years, and when excavated the plates still held the preserved bones of birds, fish, squirrels, and other small animals.

E: With perfectly preserved skeletons to observe, the position of the pottery and other artifacts buried with this Indian chief was never in doubt. Analysis of bones such as these shows them to contain up to 20 per cent $CaCO_3$, or about twice their normal content.

TABLE 1

CALCIUM CARBONATE CONTENT OF NATURAL BONE AND PETRIFIED BONE SAMPLES

Nature of Bone	Source	Probable Age	Calcium Carbonate (%)
Toxodon rib (ancient rhinoceros, well preserved)	From Ocu, R. P.	About 200,000 years	5.76
Deer femur (well preserved)	Found about 2 ft. deep in shell mounds near Anton, R. P.	Unknown; probably over 400 years	6.15
Fish vertebrae (well preserved)	Same mounds as deer femur	Same as deer bone	3.47
Turtle shell bone (well preserved)	Same mounds as deer femur	Same as deer bone	21.38
Human bone (crumbling and powdery)	Found in burial urn in a dry cave on Chame Mountain, R. P.	Unknown; probably several hundred years	0.30
Galápagos goat skull (slightly weathered)	Found on sandy beach, South Seymour Island, Galápagos Archipelago	Estimated exposure to the elements, 3–4 years	7.25
Cow jaw (well preserved)	Found on plains of Penonome, R. P.	Estimated exposure to the elements, less than 2 years	4.38
Fragments from midsection of humerus of man (perfectly preserved sample)	Found in Indian graves near Venado Beach, C. Z. Bones were covered by about 2 ft. of yellow shell-bearing sand. Above the sand, and making an abrupt contact with it, is a 4–5-ft. layer of humus and coarse sea shells	Thought to be over 500 years	10.27
Fragments from end section of above humerus	Same bone as above	Same as above humerus	10.10
Skull fragments from same man as humerus above (well preserved)	Same conditions as humerus above	Same as above humerus	20.77
Miscellaneous fragments: Skull, heavy bones, and foot bones (preservation incomplete)	From yellow sand layer at Venado Beach (described above). The bones in this case were in the yellow sand but very close to the junction with the black humus and shells. Fragments fairly well preserved but apparently from bones that were mostly disintegrated	Thought to be over 500 years	16.33
Skull fragments of man (preservation poor)	From Venado Beach, C. Z. Found at junction of yellow sand and humus but in direct contact with the black soil. Skeleton totally disintegrated except the skull fragments and a few pieces that appeared to be arm bones	Thought to be over 500 years	7.19
Bird bones	Found on plates and in small pots that are usually placed in a large urn and buried with the ancient Venado Indian	Same as other Venado bones	10.60

tion as to whether the body was flexed, laid out at full length, or buried in a sitting position could be answered by direct observation instead of speculation.

The degree of petrification necessary to preserve a bone in a tropical climate seems to be in doubt. The minimum calcium carbonate content necessary to prevent a bone from crumbling after it begins to lose its original lime is apparently unknown. The question as to whether the original calcium carbonate remains in the same physical state as the bone ages, or recrystallizes as a cementing agent, still needs to be investigated. From general observation it was obvious that even the tropical environment occasionally produces chemical reactions that cause bones to be preserved in spite of the strong disintegrating processes that are always tending to destroy them.

It was hoped that a chemical analysis of a number of samples of natural, fossil, and petrified bones, which had been found under a variety of conditions, might yield some information that would contribute to the answers to the above questions. The chart of analyzed samples (Table 1) raises more questions than it answers, but it does offer material for some very interesting speculations on the subject.

Volumetric analyses were made by grinding,

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oven-drying at 100° C, and weighing about 2-g samples on an analytical balance. Fifty ml of half-normal HCl was added to each sample; then it was digested in a covered flask on a hot plate below the boiling point of the solution. When effervescence of CO_2 was complete, the solution was titrated with half-normal NaOH, using phenolphthalein as an indicator. Qualitative tests for the presence of the ferrous ion, the ferric ion, and the magnesium ion were applied to each sample. Where any of these ions were present, the above method was considered unreliable, and the sample has not been listed in Table 1. Where the above ions were not present, the equivalents of calcium carbonate were considered to be equal to the loss in the equivalents of the half-normal HCl.

This method of analysis may be criticized as having several possible sources of error. However, the calcium carbonate content of different parts of the same bone has been found to vary more than 50 per cent, so a more refined and time-consuming method of analysis would offer little advantage in making general comparisons such as these.

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THE PLACE OF PLANT PHYSIOLOGY IN AGRICULTURAL RESEARCH*

PLANT physiology, the science of the study of plants from the operational and functional viewpoint, includes the investigation of the internal mechanisms by which the plant carries on its many, complex chemical processes, the influence of environmental factors on plant behavior, and the relationships between plants.

The knowledge gained by plant physiologists in the pursuit of their science has a very practical and important field of application to human welfare, namely, in the field of agriculture. The study and improvement of agriculture are in fact primarily questions of the application of the broad principles of certain sciences, including plant physiology, to the specific problems of crop production. Agricultural research in many of its phases might therefore be called applied plant physiology.

The application of principles derived from plant physiological research has resulted in significant technological advances in agriculture over a period of at least seventy-five years. Some of the advances have been important from the standpoint of improved yields and lowered costs of production in terms of human labor. Thus our present relatively thorough knowledge and control of the nutrition of field crops, as well as recent developments in weed control, illustrate how technological progress in agriculture has grown directly from the application of the principles of plant physiological research.

Agriculture today is still the world's biggest business. Agricultural research is also big business, carried on in the interests of big business. The application of plant physiology to agriculture is big business. But, as plant physiologists know, research in plant physiology is not such big business. Let us look at this phenomenon and consider what suggestions may be made as to the future of research in plant physiology.

The outstanding fact about plant physiology in the United States is that the vast majority of present-day research is in applied fields. Although, on the basis of available information, accurate estimates cannot be made, still it appears from data collected in 1949† that the great bulk of effort in plant physiology is specifically directed toward solution of immediate problems, and that relatively much less is directed toward the development of new and general principles. It is reassuring to know that there are so many important practical problems that need immediate resolution through the application of plant physiology, but it is less reassuring to realize that enthusiasm for application may outrun development of new principles to apply. And this is exactly the predicament in some fields of applied plant physiology. Not long ago I attended a meeting of agricultural workers concerned with the development of a program for applied research on a certain agricultural problem. A serious question was involved, since very little basic plant physiological work has ever been done in the field. This incident is typical of the situation, by no means unique, in which little or no support has been available for the study of basic principles but in which abundant support is available for the application of nonexistent principles. It is not necessary to belabor the point that basic research is an essential preliminary to applied research. The distinction between science and technology is clear to most of us, as is also the intimate relation between science and technology. Nevertheless, this distinction is not always clearly drawn, and in agricultural research at present there appears to be

* Based on the address of the retiring president at the Columbus meeting of the American Society of Plant Physiologists, September 1950.

† *Plant Physiology*, **25**, 529 (1950).

some disparity between science and technology. This becomes all the more evident if one considers specific fields of investigation. All agriculture, and indeed most of life, are dependent on photosynthesis, but even today only a relatively small number of investigators are studying this fundamental problem. It is significant, too, that some of the most notable advances in our understanding of photosynthesis have been made by chemists, with financial support not from agriculture but from other sources. Another basic and general agricultural problem is that of biological nitrogen fixation—which has been investigated extremely elegantly, to be sure, but by only a very small number of workers in a very small number of laboratories. The list could be extended, but the point has already been made clear—work on the basic and general problems having to do with all of agriculture is not at present necessarily promoted on the same broad scale and with the same over-all energy that is devoted to technological application.

One might argue that research toward a complete understanding of the mechanics of photosynthesis, nitrogen fixation, or of any other problem of plant science should not be supported as agricultural research because it is not clear that such an understanding would lead to agricultural application of any consequence. Most of us would probably agree, however, that such an argument would be unsound and without support in the history of science and technology. We cannot predict in advance which basic study will lead to immediate application and which will lead to application only after some longer period. Of one thing we can be sure, however, and that is that the spectacular and unforeseen developments of technology will come from new principles established through research whose only aim is the development of understanding, the development of new principles.

It would appear, then, that there may be some inequality in certain fields between the amount of research carried on in basic plant physiology and the amount of work in what we might call the technology of plant physiology. It has been suggested here that continuing success in plant physiological or agricultural technology must spring from principles developed by basic research in plant physiology. Let us ask ourselves why it is that such basic research is carried along at a relatively low level as compared with the levels of agricultural research in general and as compared with the need for it. Is it the fault of plant physiologists? Do they too frequently select and emphasize programs that lead nowhere? Do they lack the ability to convince society in general and administrators in particular that broad programs of basic research would lead to enlarged scope for technological application? Or does the fault lie in the organization of research in agriculture and in the way in which funds available for agricultural research must at present be administered? It is my belief that all these questions must be answered in the affirmative. Any enlargement and intensification of basic research will require the cooperation of plant physiologists, and perhaps changes in the administration of agricultural research.

It is true that in the past plant physiologists have sometimes chosen to work in fields that have not turned out to be particularly fruitful in ultimate technological application. The work may well have been of value, but it has nevertheless not tended to increase the stature of basic research in plant physiology as a foundation of technological progress. Basic research in plant physiology is not, however, *ipso facto* nonuseful, as seems sometimes to be assumed in agricultural circles. We cannot be sure that any particular program will lead to results that will prove of value in technological application. We can, however, try to direct our basic or long-range research effort toward problems an understanding of which would at least bear on some technological question. The plant physiologist should perhaps attempt to formulate his problems as questions, the answers to which would truly illuminate the behavior of plants. The plant physiologist, and indeed every scientist, should be sure that the answers to the questions he poses for himself will not be trivial.

By and large, the difficulties that confront the plant physiologist today derive from causes other than past errors. They are the result, perhaps, of the fact that no one, no group, no organization, no agency, has as its clear duty and responsibility the general financial support of long-range research in plant physiology. Who is to pay for such research? That is the key question. We know that a very considerable amount of the basic work done in plant physiology is done in universities. This is supported in part by funds available to the universities for their academic obligations, of which one is the conduct of research, particularly in connection with advanced training of graduate students. Such work demands money, usually more than is at hand. Since there is no general source of funds for support of basic research in plant physiology, the university must try to get it from other sources. An anomalous state of affairs has now developed in which work in plant physiology is often supported by funds earmarked for medical

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research. Such funds include grants from the semipublic agencies that have to do with research in and alleviation of particular human afflictions, as well as from the U. S. Public Health Service itself. There is nothing wrong in the diversion of such funds to plant physiological research in instances in which the work to be done will help to solve the medical problem involved. It seems to me, however, that basic research in plant physiology logically deserves financial support from funds allocated specifically for agricultural research, just as basic medical research deserves and receives support from funds allocated to it. The plant physiologists of the universities have, however, no agencies analogous to the semipublic medical ones or to the Public Health Service, to which to go for assistance in long-range programs.

A major site for basic research in plant physiology is in the agricultural colleges and experiment stations. In certain instances agricultural colleges have made impressive records in conduct of important basic research in plant physiology. In the agricultural colleges, and particularly in the experiment stations, the pressure for technological work is frequently an important influence in the planning of research programs. It is entirely proper that applied research be done in the experiment stations. This is one of their functions, and they have performed it admirably. The present stress on applied rather than on basic research in plant physiology is, however, due primarily to the agricultural colleges and experiment stations. It is merely a question of what balance between long-range research and technological application of present knowledge should be preserved in order to assure continued progress in agriculture.

The third major factor in plant physiological research is, of course, the U. S. Department of Agriculture, which is charged primarily with applied and technological research so far as plant physiology is concerned, although important basic research is also carried on in certain fields. In the USDA, as in the experiment stations, factors having to do with the way in which funds are appropriated, as well as other forms of outside pressure, tend to place emphasis on research programs with obvious and immediate technological value.

In the United States we have developed facilities and support for agricultural research that are probably unparalleled. These facilities, and particularly the financial support, emphasize the applied phases, so that in plant physiology applied crop production research very largely dominates the scene from a quantitative standpoint. Research aimed at increasing our understanding, and providing new principles, new concepts, new material to apply, is carried forward on a much smaller scale, and on a scale that seems to be disproportionate to the significance of the problems involved. The reasons for this appear to be largely financial. Those of us from universities, or colleges of agriculture, have undoubtedly all had the experience of hearing, in discussion with a worker or administrator in applied agricultural research, "We in applied work must depend on you in the universities for basic research." Possibly. But, even supposing this is true, where is the financial support to come from? In plant science research we have no general mechanism for the financial support of basic, disinterested, long-range research the aim of which is the elucidation of principles. In this respect we have lagged sadly behind our colleagues in medicine, where events have forced the recognition of basic research, even on plants, as an integral part of all research programs.

In outlining the place of basic plant physiological research in agriculture, it has not been my intention to paint a dark picture of the present status of the science. We all recognize that a very significant amount of important work is being carried on today by the agencies responsible for agricultural progress, as well as by the universities. There has been a conspicuous intensification of such research since World War II. The point I have tried to make is that basic research in plant physiology is an integral part of agricultural research, not a luxury to be put off to some time in the future. It is not a frivolous sort of thing indulged in only by impractical professors, but is, rather, the stuff of which new technological progress is made. The future of basic research in plant physiology depends, however, not on the plant physiologists alone, but upon our establishing a clear-cut policy about who is to pay for it.

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Erratum

Owing to the necessity for a last-minute change of page make-up in the May issue the names of the signed letters on page 344 do not conform with the names in the Table of Contents. Instead of reading *Joseph A. Babor, S. C. Florman,* and *Sydney Steele,* they should read *Ernest Brennecke, S. C. Florman,* and *Sydney Steele.*—Eds.

BOOK REVIEWS

EVOLUTION IN PROCESS

Of Societies and Men. Caryl P. Haskins. xiv + 282 pp. $4.50. Norton, New York. 1951.

THIS book is an important contribution toward the solution of one of the knottiest problems of scientific thought—that of the relationships between man the biological species and man the creator and creature of his unique social and cultural heritage. Biological man is a product of a long process of evolutionary development, brought about by a combination of the same fundamental causes—mutation, gene recombination, natural selection, and genetic drift—which have shaped the evolution of other organisms as well. And yet man's social and intellectual life is based on his possession of specifically human capacities, such as the use of symbols, which are barely adumbrated in other animal species. From the time of Darwin on, many biologists and philosophers have attempted to resolve this ostensible conflict between biological and social man. These attempts floundered because of their one-sidedness, and their fiasco has left a legacy of skepticism bordering on agnosticism, especially in the minds of social scientists. Indeed, the attempts of the adolescent biology of the Victorian Age to deduce principles of ethics from real or imaginary biological laws seem at present a little ridiculous. Even if it were true that the main principle of biological evolution is "eat or be eaten" (which is quite certainly not true), it would not follow that human behavior and human evolution must of necessity be guided by the same precept. On the other hand, the opinions of some sociologists and philosophers that biology is vastly irrelevant to the understanding of man and his works is equally indefensible; this opinion constitutes a relapse into a dogmatism that belongs to the prescientific age. For no matter how unique man's position in nature is, he occupies that position by virtue of his singular biological endowment which arose in the process of evolution from ancestors that did not possess it. It is important to discover the mechanisms of this evolutionary process; their discovery might enable man to manage the subsequent evolution of his species along lines he would consider good and desirable.

The main thesis of Dr. Haskins seems amply justified by the available evidence. It is that the appearance and development of the social organization among animals as well as among men are an outcome of the process of evolutionary adaptation to the environment. But Dr. Haskins is fully aware of the pitfalls of excessive biologism. Neither natural selection nor any other known biological principles account for human history. There is no biological force or biological predestination that would carry human evolution always in the right direction. Freedom and coercion, democracy and totalitarian tyranny, are all within the range of possibilities for man. He must choose between numerous courses that lie equally open before him. After all, it is at least thinkable that man may see fit to direct his evolution toward his own biological breakdown. Although knowledge of biology should play a role in the choice, the issue will be decided by what men consider to be the greatest good—ultimately by their ethical and aesthetic feelings.

Comparisons of human and of animal societies have often been made. There exists a vast literature dealing with the social behavior of many animals. Ants, termites, bees, and other social insects have attracted a great deal of attention from many students. Dr. Haskins is the author of several important contributions in this field, and his account of insect societies is both superbly competent and concise. Most of his predecessors emphasized the similarities, often tenuous and superficial, between the human and the nonhuman societies, but Dr. Haskins stresses the basic differences between them. The marvelous organization of an ant or a termite nest justly excites our wonder and admiration, but it does not follow that we should regard the colonies of ants or termites as ideals toward which human societies must strive. Anthropomorphic interpretations of the termite societies are bad enough, but termitomorphic interpretations of human societies are even worse. The quaint naïveté of so many biologists in these matters has made sociologists wary of all biological reasoning about mankind. But Dr. Haskins is a biologist who shows quite convincingly that the human and the insect societies are biologically very dissimilar phenomena. Moreover, it is this very dissimilarity that is most revealing.

Man can exercise choice between various courses of action. This ability is evidently conditioned by his biological heredity, since abilities of this kind are absent in other organisms. Termites, ants, or monkeys are not free to decide the future of their species. Their behavior in any given environment is circumscribed within rather narrow limits by their genetic constitution. The genetic determination of behavior is much less rigid in man. It is often contended, even by biologists who should know better, that since different breeds of dogs or horses differ genetically in their behavior and capacities, different breeds of men must similarly differ in their mentalities. This reasoning is fallacious; the degree of rigidity of the genetic determination of mental traits in a species depends upon its evolutionary history. The behavior of ants or termites is fixed by their genes, because it happens to be conducive to the survival and spread of their species in the environments and under the circumstances in which they normally occur. The plasticity of the behavior, the ability to choose and plan in accordance with the circumstances that differ in different places and at different times, have proved to be of high survival value in the history of the human species. Genetic fixation of behavior has been discriminated against by natural selection in human evolution.

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The great philosopher Nietzsche thought that man, and particularly modern man, differs from animals by being conscious of history. But, in the opinion of Nietzsche, this consciousness is man's great misfortune and disability. A biologist must certainly disagree with so pessimistic a view. The history of mankind is our cultural heritage, and the possession of this heritage is manifestly an adaptive trait of great importance in the control of the environments on earth. The transmission of the cultural heritage presupposes the ability to teach, to learn, and to modify one's actions in accordance with the knowledge and wisdom acquired in the process of learning. The educability, the plasticity of behavior, is adaptively by far the most important biological quality of the human species which has been developed and maintained by natural selection throughout human evolution. The achievement of freedom joined with responsibility which is fostered by the democratic social organization may, in this sense, be said to represent the highest development, so far, of this specifically human evolutionary tendency.

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DREAM OR REALITY?

Men of Other Planets. Kenneth Heuer. x + 160 pp. Illus. $3.00. Pellegrini & Cudahy, New York. 1951.

THIS is a gracefully written little book about life on other worlds; the air, attitude, and mood are that of a gently hopeful lecturer, speaking to children and elderly ladies. There is a certain air of gentle fuzziness about it; it is pleasant material, but it lacks the sharp clarity of thought and presentation that would have made it a genuinely scientific text. There is mention of radar contacts with the moon, of rocket ships and atomic jet drives—but the viewpoint is mainly determined by astronomical work of the nineteenth century. The harsher verdicts of the twentieth century, based on observational work done with the larger instruments, the more precise methods of bolometer and infrared spectrometer, are largely by-passed for the kindlier judgments of the earlier and more hopeful-of-other-life observers of the last century.

It is true that the *author's* speculations are clearly marked as such, but the opinions of earlier astronomers are not quite so clearly marked as educated guesses based on inadequate data. Russell's work on planetary atmospheres, the recent *Atmospheres of the Earth and the Planets*, have been consulted much less than Pickering and Lowell.

The general level of the book seems based on an effort to please, interest, and inspire the young and the old—but not the middle group who prefer their material sharply focused, and without the softening haze of "we can't prove it isn't so."

There is much sound material presented, and for the teen-ager, the book can be a gold mine, something to start a mind speculating, wondering—and seeking further into the infinite and fascinating field of the deep space beyond our shallow atmosphere. For the already developed scientist, the book is disappointing; for the mind that may be turned to science tomorrow, the book is provocative and well done. The one objection at that level is that there are also alluring possibilities derived from the most recent knowledge of planetary structures that could have been worked in, even if that knowledge does rather rule out locust swarms on the moon, and inhabitants of 300-mile-diameter planetoids.

The latter part of the book, dealing with the position of the earth and solar system in space, is more direct and more clearly expressed; all this material has, necessarily, been taken from work done in this century, since the structure of the galaxy had not been elucidated until recently.

JOHN W. CAMPBELL, JR.

Astounding Science Fiction
New York City

A PHYSICIST'S AUTOBIOGRAPHY

From the Life of a Researcher. William Weber Coblentz. xi + 238 pp. $4.75. Philosophical Library, New York. 1951.

THIS is the autobiography of a scientist who has spent practically all his adult life as a member of the staff of the National Bureau of Standards. His specialty has been thermal radiation and the development of instruments for this purpose. His book is the outgrowth of a requirement of every member of the National Academy of Sciences to place on file autobiographical facts not printed in current biographical reference books—such as the environment under which he was reared, his first recollections and juvenile behavior—which may throw light on his adult performance.

Nearly half the book is devoted to the early days of the writer, and contains much interesting reading. A later long chapter is devoted to a summary of his physical investigations and some applications of the results. His early investigations on the absorption spectra of the few colored glasses then manufactured found an application during the first world war in protecting the eyes of welders (and also the enshrined Declaration of Independence) from injurious ultraviolet radiation. He also studied the light emitted by fireflies, and the result enabled biologists to discover that the light-emitting organs of the firefly contain two substances, one of which generates the light and the other emits it. Many other investigations and their applications are described in this chapter.

Another chapter will probably be of equal interest. It contains a summary of the author's investigations of psychic phenomena. This side of Dr. Coblentz' activities was practically unknown to his colleagues at the National Bureau of Standards. The reader must be left to form his own opinion of this part of the book. In doing this he may find it useful to refer to an article entitled "Telepathy—a Survey," by Sumner Boyer Ely, pub-

lished in THE SCIENTIFIC MONTHLY, in February 1940. In this connection there may be cited the parallel case of the British physicist Sir Oliver Lodge, who served a term as president of the Society for Psychical Research. Brooding over the loss of a son in the first world war, Lodge eventually received (as he thought) a message from his son. This intensified his interest in psychic matters, and he arranged that after his death there would be found among his effects a sealed envelope containing a message he would endeavor to transmit to his friends. Should any of these friends receive what purported to be such a message, Lodge directed that it should be enclosed in a sealed envelope and given into the care of his family. Ten years after Lodge's death all the envelopes were to be opened and their contents compared. The ten-year period ended August 22, 1950. This reviewer has seen or heard nothing of the result of this experiment. Had only one of the friends submitted a message resembling that left by Lodge it would have been matter for headlines in the newspapers.

PAUL R. HEYL

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DEVELOPMENT OF A GIANT INDUSTRY

Oil! Titan of the Southwest. Carl Coke Rister. xxiii + 467 pp. Illus. $5.00. University of Oklahoma Press, Norman. 1949.

THE publishers claim that "everyone—from the tool pusher and driller to the Wall Street investor and the car owner . . . will acquire from this book a better understanding of the vast oil industry of the Southwest, which today provides 70 per cent of the oil produced in the United States." Perhaps the claim is accurate, but the fact is that it will take someone with an interest in the oil industry to plod through the minutiae of explorations, discoveries, and developments, from the first press notice of oil in Kansas (March 31, 1855—or four years before Drake brought in his well at Titusville, Pa.) to the bringing in of the discovery well in East Texas on October 14, 1929.

Admittedly this limitation still leaves the book a large potential public, for in much of the Southwest, and especially in Texas, Oklahoma, and Louisiana, it is as difficult to find the inhabitant who is not interested in oil as it is to single out the Philadelphian who allegedly does not read the *Bulletin*. In the oil country every detail connected with drilling and discovery is weighted with significance, and Carl Coke Rister succeeds in reproducing the inconsistent and almost breathless loss of perspective that is characteristic of the oil country. He fails to impart life to the raucous boom days and the wild boom towns, but he has captured something of the purposeful confusion of oil pool development.

The chronology of events is given geographic order as Rister moves from the accidental but portentous discovery of oil at Corsicana in 1894, and the scientific designation of the drilling site at Spindletop, which came in on January 10, 1901, to the discoveries in the Permian Basin, the Panhandle, and East Texas. Later developments are sketched in rather general terms, for, as the author notes, discovery, like development, has of necessity become too much of a science and too heavy in investment for all but the lucky rugged individual. Happily, the latter still exists; and, despite the fact that the production, refining, and distribution of petroleum dominates Southwest industry, without quite as much altruism as the author implies in his final chapter, the individual wildcatter, who puts his last dime into a wildcat well, is the most important adjunct in developing the new reserves needed to offset the unprecedented annual drain.

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HYGIENE

The Science of Health. (2nd ed.) Florence L. Meredith. xiii + 452 pp. Illus. $3.75. Blakiston, Philadelphia. 1951.

THOUGH "Second Edition" is printed at the head of its title-page, this volume is in fact the *third* edition of the outline known in 1935 as *Twelve Hours of Hygiene*, an abridgment of the author's larger textbook on *Hygiene* (1st ed., 1925; 2nd ed., 1932). Under its present title the outline was first published in 1942. The purpose of the "second" edition remains the same as that of the 1935 abridgment—to serve as a textbook in one-hour, one-semester college courses of hygiene. The author is eminently qualified to write textbooks for such courses because she is a physician who has long experience as lecturer and professor of hygiene at several colleges (Tufts, Simmons, etc.). She also proves herself a good writer who has an easily read style and a sense of values that have helped her in the collection of adequate material and in the instruction of well-balanced knowledge.

There are many technical editorial innovations in this issue of the work, such as transfer of paragraphs to more suitable chapters, new titles for chapter heads, enlargement of sections into new chapters, grouping of all chapters into four parts, and merger of the end tables with the corresponding portions of the text. Part 3, "Major Health Problems in the United States," is fully rearranged. Formerly it was just an alphabetical list of a few major internal diseases, from anemia to ulcer of the stomach. Now there is a seemingly new Chapter 12 on communicable diseases, and the rewritten text on noncommunicable diseases makes up Chapter 13.

The statistical references seem to run up to 1948, but the list of recommended reading matter stops short at 1946 (except for a few Blakiston publications). There are a number of new illustrations, though some of the anatomical pictures date back to textbooks printed in the 1920s. The part on endocrine diseases (pp. 296–302) could stand further additions and refreshment. The type and the general appearance of the book are very pleasing.

CLAUDIUS F. MAYER

Index-Catalogue
Army Medical Library, Washington, D. C.

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SURVEY OF LOCAL FAUNA

Handbook of Amphibians and Reptiles of Kansas. Hobart M. Smith. 366 pp., 233 figures. Apply, adding 25 cents to cover postage, to University of Kansas Museum of Natural History (Misc. Pub. 2), Lawrence. 1950.

THIS new herpetological survey is an exhaustive popular guide from the prolific pen of brilliant, indefatigable Hobart M. Smith, now at the Museum of Natural History, University of Illinois. It is a magnum opus that any state faunal survey group would be proud to produce. Most will remark that it is just the sort of thing they need, but, they will regretfully add, cannot afford to produce.

The handbook results from a continuation of the work of the Kansas Biological Survey begun by C. D. Bunker, of the Museum at Lawrence, and has been published by the state in "response to scores of requests from citizens of Kansas for information concerning the amphibians and reptiles of the State."

The striking color photograph of the copperhead on the cover is an appropriate sign of the contents. Dr. Smith confesses that "Little that is new is included in these pages," but the text concerning each species (range, description, recognition characters, habits, and habitat) is concisely drawn from a wide command of original and published data. Such little-known facts about the common species as his statement "There is considerable evidence that any one female common water snake (*Natrix sipedon*) will bear young only in alternate years" abound in the book. In fact, the lack of a complete coverage of all the available life history data indicates the trend nowadays to cater to the layman instead of strictly to the scientist.

A general historical account of herpetology in Kansas is actually a tribute to such pioneers as Hallowell, Cragin, Hammond, Branson, Taylor, Burt, Gloyd, and Bunker. Pertinent material is well presented for venomous species, collecting, care of live individuals, preservation of specimens, identification, and other general instructions. Maps are an important and welcome addition to distribution information—a small map of the United States accompanying each large state map demonstrates the general distribution. The former has the appropriate dots showing collections records. Excellent, finely reproduced photographs are a decided asset, particularly so since they are printed on a moderately glossy high-grade paper. The lizard photos, taken from Smith's *Handbook of Lizards,* are better reproduced than in the original. Some of the salientian photos, from Smith's *Amphibians of Kansas,* are, on the contrary, inadequate from the standpoint of observing pattern and detail.

Excellent keys from higher to lower categories of amphibians and reptiles are not presented as a single unit. Experience in the classroom indicates that many people must start from scratch; consequently, the key in its entirety might greatly facilitate "running down" a species. The layman's disinterest on subspecific taxonomic units has not been overemphasized, as when Dr. Smith states, "It seems to be unnecessarily confusing, for most beginners, to be confronted with the details of subspecies identities." More of his colleagues might take a lesson from this when educating beginners. Nevertheless, at the end of each account of amphibian or reptile, he includes a brief discussion of each subspecies breakdown. This is somewhat complicated, however, by the use of the accepted common name of *most* subspecies, with no explanation for the remaining ones.

The herpetofauna of Kansas is composed of 97 species, or a total of 105 species and subspecies, in which the following kinds are included: salamanders, 11; anurans, 21; lizards, 16; snakes, 44; turtles, 13. Thirteen other unverified species and subspecies are noted under the hypothetical list. A bibliography for each species is placed at the end of each species' biography, in addition to a list of 176 references. A glossary and index contribute to the completeness of the work.

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FISHER'S COLLECTED PAPERS

Contributions to Mathematical Statistics. R. A. Fisher. Approximately 1,200 pp. (not consecutively numbered). Illus. $7.50. Wiley, New York; Chapman & Hall, London. 1950.

THIS large and well-printed volume combines photographic reproductions of 42 papers selected by R. A. Fisher from his work and a brief description of three other papers that could not be included in the collection. The whole covers the fruitful work of Fisher extending from 1915 to 1942. Along with the recent volume of selected papers by Karl Pearson (*Karl Pearson's Early Statistical Papers,* Cambridge University Press [1948]) the volume under review contributes much to the facilities for studying trends in statistical research and constitutes a valuable addition to the series of statistical books published by Wiley & Sons under the editorship of Walter A. Shewart.

A very able "manipulative" mathematician, Fisher enjoys a real mastery in evaluating complicated multiple integrals. In addition, he has a remarkable talent in the most difficult field of approaching problems of empirical research. As a result, his lifework includes a series of valuable contributions giving exact distributions of a variety of statistics, such as the correlation coefficient, the central χ^2 with due allowance for degrees of freedom, the noncentral χ^2, the quotient of two χ^2, etc., etc. These distributions are bound to stay on the books and be used continuously. In this connection, it is regrettable that the volume does not contain Fisher's paper giving the distribution of the correlation coefficient published in *Biometrika* in 1915. It may be hoped that the editor of *Biometrika* will reissue the reprint of this paper so that it can be appended to the present volume.

In his long scholarly work Fisher also made frequent

attempts at the conceptual side of mathematical statistics, and these efforts are duly reflected in this volume. In particular, three major concepts were introduced by Fisher and consistently propagandized by him in a number of publications. These are mathematical likelihood as a measure of the confidence in a hypothesis, sufficient statistics, and fiducial probability. Unfortunately, in conceptual mathematical statistics Fisher was much less successful than in manipulatory, and of the three above concepts only one, that of a sufficient statistic, continues to be of substantial interest. The other two proved to be either futile or self-contradictory and have been more or less generally abandoned.

Fisher's great achievement is his contribution to (really the founding of) the theory of experimentation. Here, the idea of *a design* and the many ideas of particular designs, including randomization, are important conceptual contributions. They are skillfully combined with the manipulative layout of the analysis of variance and constitute a useful tool for the experimenter in a great number of fields of research. This part of the scholarly work of Fisher, somewhat on the fringe of mathematical statistics, is reflected mostly in Fisher's books and is not adequately represented here.

As already emphasized, Fisher has contributed many important results, and many of these are contained in the papers reproduced in the present collection. If this volume were limited to reproductions of these papers, it would constitute a real monument to Fisher's scholarly activity, of which he and his friends could be justifiably proud. Unfortunately, preceding each paper there is a prefatory note by Fisher. The majority of these are polemical, although in many cases this may not be apparent to the casual reader. They are an integral part of the volume, and, since some of them tend to harm other scholars, it appears appropriate to give them some attention.

The prefatory note on page 29.302a contains a personal attack on Karl Pearson. Its tone may be exemplified by the following passage: "If peevish intolerance of free opinion in others is a sign of senility, it is one which he had developed at an early age. Unscrupulous manipulation of factual material is also a striking feature of the whole corpus of Pearsonian writings, and in this matter some blame does seem to attach to Pearson's contemporaries for not exposing his arrogant pretentions."

Other prefatory notes are not so brutal in form, but many of them are insidious because, in a skillfully hidden manner, they involve unjustified claims of priority. Two examples of this kind must suffice.

The very first prefatory note describing Fisher's paper of 1915 on the correlation coefficient (not reproduced in the volume) contains the following sentence (p. 1.2a): "Here the method of defining a sample by the coordinates of a point in Euclidean hyperspace was introduced. . . ." This sentence, in a delicate and convincing manner, puts forward a claim for priority with respect to a useful method. This claim has been made by Fisher before and has been accepted by many of his readers. Thus, for example, it is acknowledged by Mahalanobis in his biography of Fisher which is reproduced as the first item in the book (p. 268, line 7 from the bottom). However, the same method of representing the sample by a point in multidimensional space (and for a similar purpose) was also used by Karl Pearson in 1900 (fifteen years earlier) in his paper on the chi-square distribution and, probably, had been used even before that.

Another unjustified claim of priority refers to the so-called property of efficiency of the maximum likelihood estimates. According to Fisher's terminology, a consistent estimate is called efficient if it is asymptotically normal and if, compared with all such estimates, it has the minimum asymptotic variance. In his paper of 1908 (*J. Roy. Statist. Soc.*, **71**, 651–78, particularly 662 [1908]), F. Y. Edgeworth conceived the idea that maximum likelihood estimates (Edgeworth used another term) are always efficient and, with the aid of Professor Love, made several attempts to prove his conjecture. Firmly convinced of the truth of his theory, Edgeworth went further and introduced the idea of rating the precision of other asymptotically normal estimates by the ratio of their asymptotic variances to the asymptotic variance of the maximum likelihood estimate. This theory was then applied to the study of the efficiency of the method of moments in relation to fitting Pearson curves.

Some thirteen or fourteen years later the same idea was conceived by Fisher, apparently independently of Edgeworth, and proclaimed with great emphasis in his papers of 1922 and 1925 (Nos. 10 and 11 in the present volume). Since then, the assertions that the maximum likelihood estimates are always efficient and that this discovery was made by Fisher have become generally accepted and have been repeated in many textbooks. Actually, the proofs of the efficiency of maximum likelihood estimates offered both by Edgeworth and by Fisher are inaccurate, and the assertion, taken in its full generality, is false. Nevertheless, in cases of asymptotic normality, maximum likelihood estimates do minimize the asymptotic variance compared with a somewhat narrower class of alternative estimates. Therefore, Edgeworth's conjecture proved useful and important. As mentioned, it is likely that, when writing his papers of 1922 and 1925, Fisher was not aware of Edgeworth's results. However, in 1934 Bowley called Fisher's attention to the fact that some of the results claimed by him had already been published in Edgeworth's paper of 1908 (*J. Roy. Statist. Soc.*, **98**, 56 [1935]). Fisher's reply to Bowley (*ibid.*, 77) is a masterpiece of polemics, and the claim of Edgeworth's priority was neither granted nor denied. By implication, however, Fisher admitted having read the paper by Edgeworth.

Since the results of Edgeworth (1908) described above form a very substantial part of Fisher's paper of 1922, one would think that the prefatory note used with this paper (or, perhaps, with the paper of 1925) republished in the present volume would contain some

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reference to Edgeworth. It does not. Instead it contains passages like the following (p. 10.308a): ". . . Consequently, one purpose of the paper is to examine the properties of the likelihood function, here defined, and the properties of the estimates arrived at by maximizing this function. Several fruitful ideas, such as the measurement of the amount of information, emerge. . . ." No mention is made by Fisher that, some thirteen years previously, the same idea "emerged" in the mind of Edgeworth (*loc. cit.*, 678) the only difference being that Edgeworth's measure is the reciprocal of that of Fisher and does not bear the picturesque label of "amount of information."

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LEPIDOPTEROLOGY

A Field Guide to the Butterflies of North America East of the Great Plains. Alexander B. Klots. xvi + 349 pp. + 40 plates, 16 in full color. $3.75. Houghton Mifflin, Boston. 1951.

NOT since the publication of Scudder's classical work in 1889 have our Eastern butterflies been treated in such a thorough and scholarly manner as in Professor Klots' *Field Guide*; and no other book on North American butterflies can be compared to it. Intensive work in the field over a large part of North America, supplemented by detailed and long-continued study in the laboratory, has given Dr. Klots an intimate knowledge of all aspects of our butterfly fauna equaled by few other workers—if any. Our fauna, in large part a northerly attenuated extension of that of tropical America, cannot really be appreciated unless one is familiar in the field with the latter. Dr. Klots has collected and studied butterflies in some of the richest areas in South America; also, he is far more familiar than most lepidopterists with the fauna of Europe and Asia.

The *Field Guide* is much more than its title would indicate. It is invaluable for the professional quite as much as for the amateur. Brief but adequate descriptions of all the species are supplemented by notes on the occurrence, seasons, early stages, and food plants; the ranges are accurately given. Subspecies are quite properly relegated to a subordinate position. The numerous figures, both colored and black-and-white, are excellent. The distinguishing features of most species are indicated by leading lines, which is very helpful, and the locality of each figured specimen is given.

In addition to the accounts of the individual species, there is a detailed introduction that includes information on the collecting and preservation of specimens, and a very fine discussion of habitats and habits of butterflies, their relation to their environment, their life history, their structure and colors, and their classification. At the end of the book there is an essay on principles of classification, a brief history of the study of our butterflies, a list of the more important collections, a well-selected bibliography, and a check list of all the butterflies of eastern North America. The indices are especially good.

For a book with such complex matter and involving such a multiplicity of detail, errors are surprisingly few. On plate 14, page 113, Figure 14 should have been numbered 12, Figure 12 should be 13, and Figure 13 should be 14; on plate 28, page 225, Figures 10 and 11 are exchanged; on plate 35, page 288, the legend for Figure 7 (*Poanes hobomok*) is omitted. These are editorial errors for which the author can scarcely be held responsible.

Dr. Klots is to be congratulated on producing such an outstanding contribution to the study of North American butterflies.

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THE GENE AND YOU

The New You and Heredity. Amram Scheinfeld. xii + 616 pp. Illus. $5.00. Lippincott, Philadelphia. 1950.

The New You and Heredity is an extensive revision and enlargement of the older volume published twelve years ago. Four fifths of the book are entirely new or extensively rewritten, and the new edition is half again as large as the older one. The remarkable insight into problems of human genetics displayed in the first edition by the author, who is not a professional geneticist, has again been demonstrated in the present edition. The same friendly, intimate style is used, sobered somewhat by experience, but still carrying the reader along on a continued wave of interest. Although the criticism of oversimplification that was frequently lodged against the earlier presentation is still to some extent valid, the general reading public will find much of absorbing interest and lasting value in the book. New material includes a discussion of the Rh factors, the Russian genetics controversy, and the possible genetic effects of atomic bombing. Kinsey's studies are comprehensively considered in the chapter on Sex Life. The author's personal study of the inheritance of musical talent, which was not the most valuable part of the earlier edition, has been condensed and summarized.

Throughout the book a commendable awareness of the interaction of heredity and environment is apparent. Another commendable feature is the constant attempt, certain to be successful, to start the reader thinking for himself in terms of the social and personal implications of human genetics. As in the previous volume, the illustrations are excellent, many of them clever, and all accomplishing their purpose clearly and efficiently. The new illustrations are even better than the older ones, which is saying a good deal. All in all, the book can be highly recommended to those seeking an introductory acquaintance to human heredity.

Laurence H. Snyder

The University of Oklahoma

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American Society of Zoologists
Herpetologists' League
Society of Systematic Zoology

FG American Microscopical Society
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Biometric Society, Eastern North American Region
National Association of Biology Teachers

G Philadelphia Botanical Club

I Society for Research in Child Development

K American Home Economics Association
National Academy of Economics and Political Science
Pi Gamma Mu

L Philosophy of Science Association

N Alpha Epsilon Delta

P American Industrial Hygiene Association

Q AAAS Cooperative Committee on the Teaching of Science and Mathematics
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